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THE JOURNAL OF PHILOSOPHY

SYMPOSIUM *

DETERMINISM IN THE LIGHT OF RECENT PHYSICS

I. COMPLEMENTARITY IN QUANTUM PHYSICS AND ITS PHILOSOPHICAL GENERALIZATION

1. INTRODUCTION

BOHR'S principle of complementarity has been at the focus of the debate on the microphysical validity of determinism and on the existential status of unobserved atomic entities. Moreover, in the form of a general principle of epistemology, it has been held to provide fundamental resolutions of controversies on such issues as free will and autonomy versus reduction of biology and psychology, receiving the tribute of being "in one sense the most revolutionary philosophical conception of our day."[1]

Any assessment of the bearing of current physical theory on the tenability of determinism therefore ought to include a philosophical appraisal of the principle of complementarity and its generalization. This paper will be devoted to presenting these two evaluations. As it goes to press, the author unfortunately does not yet have the benefit of acquaintance with Bohr's Compton Lectures on "Quantum Physics and the Notion of Complementarity," which are scheduled for delivery at the Massachusetts Institute of Technology during the period of November 5–26, 1957.

2. COMPLEMENTARITY WITHIN QUANTUM MECHANICS

The epistemological and ontological upshot of the principle of complementarity can be placed into bold relief by a precise statement of Bohr's warrant for denying that the Heisenberg indeterminacy principle is merely a principle of limited measurability.

This denial could not be upheld, if it *did* rest, as we are often told it does, on some mere "operational incompatibility" of pro-

* To be presented at the meeting of the American Philosophical Association, Eastern Division, December 29, 1957. The writer of the first paper is indebted to the National Science Foundation for the support of research.

[1] J. A. Wheeler, "A Septet of Sibyls: Aids in the Search for Truth," *American Scientist 44* (1956), p. 374.

cedures of measurement issuing in a pair of sharp, simultaneous numerical values of such "conjugate" parameters as position and momentum. For valid examples have been given of what are, purely operationally, definitely performable measurements yielding specific position and momentum numbers which refer to the same instant of time. Thus, to cite an example given by Heisenberg himself [2]: suppose that a highly accurate position measurement made on a free "particle" (electron) shows it to be at point A at time t_A and that a second such measurement locates the particle at point B at the later time t_B. Utilizing one of the possible "operational" definitions of the "velocity" of a free particle of mass m in Newtonian mechanics, we could say that measurement has shown the particle's sharp momentum to have been $m\frac{\overline{AB}}{t_B - t_A}$ throughout the time interval $t_B - t_A$ and, in particular, at the time t_A for which the exact position of the particle is known.

Better still, consider the following quite possible measurement mentioned by Margenau in which the operations themselves and not merely the quantities derived from them are as nearly simultaneous as we please [3]: a gamma ray microscope is used to obtain a definite position number from an electron and simultaneously, by using waves of suitable greater length as well, a definite momentum number. The important point here is that such measurements, though entirely compatible operationally, are *not theoretically significant:* the specific simultaneous values furnished by them cannot be regarded as defining a precise state of the atomic entity productive of information concerning its past and future behavior in the manner of classical physics. Thus, in Heisenberg's example, the ascription of an exact momentum to the electron at time t_A could not be used in the classical manner to infer its behavior prior to t_A and, if used to infer the electron's position at time t_B, is merely redundant with what we already know from direct measurements. Moreover, if the exact momentum value in question were to be regarded as characterizing the state of the electron at time t_B, measurements taken after this time in repeated runs of the same kind of experiment would show this attribution to be untenable, since the observed momentum values would spread in accord with the uncertainty principle. Thus, what is wrong with the kinds of simulta-

[2] W. Heisenberg, *The Physical Principles of the Quantum Theory*, Chicago, 1930, p. 20. Cf. also A. D'Abro, *The Decline of Mechanism in Modern Physics*, New York, 1939, pp. 664–665, and P. Frank, *Philosophy of Science*, Englewood Cliffs, N. J., 1957, pp. 216–217.

[3] H. Margenau, *The Nature of Physical Reality*, New York, 1950, pp. 376–377. Cf. also that author's "Philosophical Problems Concerning the Meaning of Measurement in Physics," *Philosophy of Science* (in press).

neous measurements we are considering is *not* that their joint execution and issuance in definite numerical values is operationally impossible; instead it is that the sharp values they do yield via *compatible* operations, when both interpreted as exact values of the conjugate parameters of the theory, are at best theoretically sterile and at worst a source of false predictions. Accordingly, the Heisenberg principle must be understood as asserting that no *theoretically significant* exact measurements of conjugate values can be operationally compatible, i.e., simultaneously performable. And the examples given by Heisenberg and Margenau thus show quite clearly that the mere operational incompatibility of exact values of conjugate parameters is *not a necessary condition* for their *failure* to possess simultaneous theoretical meaning. A brief look at the structure of the sort of laboratory phenomenon that provides the *reason* for this failure in quantum mechanics will enable us to draw the following further conclusion: the *operational incompatibility* of theoretically significant exact joint values is *not* a *sufficient condition* for the lack of theoretical meaning on the part of precise simultaneous values. It will then be evident that there is even *less reason* to deny such meaning in contexts in which the mere procurement of two simultaneous data in a single experiment is impossible, a kind of operational incompatibility *not* present in quantum mechanics. And this result will turn out to be fatal to some of the attempted philosophical generalizations of quantum complementarity, as we shall see later on.

Consider the passage of electrons (or other atomic entities) toward a screen through a diaphragm containing two slits. The interference pattern resulting on the screen when both slits are open is found *not* to be the same as the kind obtained by superposition of the individual patterns produced by each one of the two slits while the other is closed. It is fundamental to appreciate the significance of this fact and of a host of others like it, wholly apart from any kind of operational incompatibility, as evidence *contradicting* the assumption that an electron can be the kind of entity possessing simultaneously a definite though *unknown* position *and* velocity. Such an entity would describe a *particle trajectory*, which though unknown would certainly be expected to be independent of whether a slit through which the particle did *not* pass is closed or open, an expectation which the interference pattern shows *not* to have been fulfilled.[4] The contribution which this sort of observational fact

[4] The class of observational findings referred to here poses serious problems for attempts such as that due to D. Bohm (cf. *Causality and Chance in Modern Physics*, London, 1957) to interpret quantum phenomena within a framework making only minimum departures from the ontology of classical physics. Bohm has endeavored to provide a deterministic substratum for quantum

makes, independently of any operational incompatibilities, to the theoretical illicitness of ascribing sharp simultaneous values of conjugate parameters to the electron becomes particularly evident by noting what our interpretation would have been if the following hypothetical situation had materialized: (a) *contrary to actual fact,* the pattern produced by the two open slits is the superposition of the two individual ones, *and* (b), just as in the actual experiment, the attempt to trace individual electron trajectories by direct measurements is operationally incompatible with the survival of the two-slit interference pattern on the screen. If the latter counter-factual situation had indeed been realized, it would have been a perfectly *reasonable induction* on the basis of the *indirect* evidence provided by the imagined superposition structure on the screen to infer that (1) although we cannot ascertain for an individual electron through which one of the two slits it passed, the theoretical ascription of simultaneous precise though unknown position and velocity values to the electron is fully warranted, and (2) a principle of limited measurability assures that any sharp measurement of the value of one of the conjugates will unpredictably alter the otherwise well-defined value of the other. This comparison of the result of the actual experiment with that of the putative one makes it plain that operational incompatibility of jointly significant sharp simultaneous values is *not* a *sufficient condition* for their lack of joint theoretical meaning. And it likewise shows that it is a serious error to suppose, as M. Bunge did in a recent paper,[5] that Bohr's rejection of the thesis of mere limited measurability derives "simply" from the fact that the relevant attributes "are not simultaneously observed." For our comparison shows that it is because these classically con-

mechanics by reviving an early interpretation of de Broglie's in which classically conceived particles are guided by pilot waves. But Heisenberg has noted (cf. his essay in *Niels Bohr and the Development of Physics,* London, 1955, p. 17) that Bohm is driven to the necessity of saying that for systems consisting of *several* particles, the ψ-waves spread, not in our 3-dimensional space but in an n-dimensional configuration space. And Reichenbach (cf. *Annales de l'Institut Poincaré 13* (1953), pp. 128–129) has recalled de Broglie's own early awareness that even in that unreal hyperspace, the behavior exhibited by the waves would be, classically speaking, every bit as strange as that of a "particle" whose motion depends on the presence of a slit through which it could not have passed. In regard to the first of these criticisms, Bohm has replied (*op. cit.,* p. 119) that work now in progress which is based not on the many-body Schrödinger equation with its hyperspace but on the 3-dimensional "second-quantized" field theory "has already gone a long way towards indicating a possible solution to this difficulty." For a review article, "The Causal Formulation of Quantum Mechanics of Particles," see H. Freistadt, *Nuovo Cimento,* Supplement to vol. 5, 1957, pp. 1–70.

[5] Bunge, "Strife about Complementarity," *British J. Phil. Science 6* (1955), p. 2; cf. also pp. 4, 6, 8, and 152.

ceived attributes do not jointly exist to begin with that they cannot be obtained by simultaneous measurement and that there can therefore be no question of a "disturbance" in the already existing value of one conjugate variable being *"caused"*[6] by the observation of the other. Instead of being simultaneous "autonomous" attributes of the microphysical object, belonging to it *independently* of the *particular experimental arrangement* into which it enters, exact theoretical values of conjugate parameters are each only *interactional* properties of an atomic object that is coupled *indivisibly* to a particular kind of observational macro-set-up. For the incompatibility of the circumstances allowing the theoretical ascription of a sharp value of one conjugate parameter with those allowing the corresponding assignment for the other renders these attributes *theoretically disjunctive* and interactional. And their "complementarity," as Bohr uses this term, comprises both their theoretical disjunctiveness and the separate need for each of them in any account of the totality of interactions.[7]

To assert that the particular pairs of attributes which furnished the mechanical state descriptions in classical physics are neither autonomous nor theoretically *con*junctive but rather interactional and theoretically *dis*junctive does *not* itself entail that there are also no *other* attributes of, say, an electron whose existence is *independent* of whether the electron is observed. If one denies the existence of any such other attributes as well, then indeed there is no articulate sense in which the electron can be supposed to exist independently of being observed. It is therefore not surprising that those quantum theorists who have embraced the latter view endorse at least one of the following two ontologies:

(i) macrophysicalism: the existents of the physical world are the macro-objects and events of classical physics. Micro-objects do not exist as such at all but can be introduced into physical theory as a kind of computational or linguistic link between specifiable

[6] *Ibid.*, p. 142.

[7] It should be noted that the properties which are being referred to as "complementary" here are those named by the values of the conjugate parameters of the Heisenberg principle and *not* such supposed properties as "being wave-like" or "being particle-like." The latter had better be omitted from quantum mechanical discourse altogether: it is a misleading carry-over from the classical interpretations of experimental data in terms of waves and particles—an interpretation whose very breakdown is the *ratio essendi* of the new theory—to suppose that an interference pattern, for example, calls for a wave interpretation, whereas cloud chamber tracks and the photo-electric effect warrant a corpuscular explanation. And thus the terms "wave-like" and "particle-like" should *not* be used as surrogates for *bona fide* complementary parameter values.

experimental arrangements and their observable consequences.[8] Accordingly, to speak of electrons, for example, as "interacting" with measuring devices has, existentially, only a Pickwickian meaning. For only the registrational processes and components of the classically-describable total experimental arrangements are existents.

(ii) The inarticulate ontology of a Kantian intersubjective idealism without retention of all the specific features of its original apriorism.

Unfortunately, in regard to the ontology which they regard to be implicit in quantum complementarity, the leaders of the Copenhagen interpretation, Bohr and Heisenberg, have made pronouncements containing personal philosophical credos irrelevantly adjoined to the statements of their complementarist thesis proper. Thus, after a lucid statement of that thesis, Bohr says: "We meet here in a new light the old truth that in our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience."[9] I should have imagined that such a conclusion would have been germane not to Bohr but to someone who is anxious to deduce a metaphysical agnosticism from a prior misinterpretation of the Heisenberg principle as a principle of mere limited measurability. In the case of Heisenberg, an espousal of the macrophysicalistic ontology is interlaced with and retracted by the championship of a Kantian homocentrism, which he regards as the complementarist alternative to "the ontology of materialism," i.e., "the idea of an objective real world, whose smallest parts exist objectively in the same way as stones and trees, independently of whether or not we observe them."[10] Interestingly, one first finds the following avowal of macrophysicalism by him:

> Of course, the introduction of the observer must not be misunderstood to imply that some kind of subjective features are to be brought into the description of

[8] This seems to me to be the ontology of the physicalism of Carnap's "Testability and Meaning," *Philosophy of Science* 3 (1936), esp. pp. 450 and 463–468.

[9] N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge, 1935, p. 18; hereafter, this work will be cited as "ATDN." We find A. Hildesheimer writing in the same agnostic vein (cf. *Die Welt der ungewohnten Dimensionen*, 2nd ed., Leiden, 1955, p. 339; also pp. 286, 293, 338–351). Then, assisted by the equivocal use of the term "observer," Hildesheimer commits the inveterate idealist error of identifying the purely material macro-apparatus which registers the results of measurements with the *mental* component of the *human* observer. And this then enables him to conclude that "quantum mechanics . . . becomes a science which, so to speak, lies between physics and psychology" (p. 327).

[10] W. Heisenberg, "The Development of the Interpretation of Quantum Theory," in *Niels Bohr and the Development of Physics* (ed. by W. Pauli), London, 1955, p. 17.

Nature. The observer has rather only the function of registering decisions, i.e., processes in space and time, and it does not matter whether the observer is an apparatus or a human being . . . in this respect the Copenhagen interpretation of quantum theory is in no way positivistic. For whereas positivism is based on the sensual perceptions of the observer as the elements of reality, the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts, i.e., the actual, as the foundation of any physical interpretation.[11] . . . [It] is indeed based upon the existence of processes which can be simply described in terms of space and time . . . and which thus compose our 'reality' in the proper sense.[12]

But, having said this, Heisenberg quickly treats us to an idealist afterthought containing a Kantian form of the genetic fallacy: he infers that the *logical* ground for the limited applicability of the reality concept of classical physics to the restricted domain of the macro-world lies in the fact that classical physics was formulated by man, a medium-sized macro-object. As he puts it:

It is of course not by chance that "objective reality" is limited to the realm of what Man can describe simply in terms of space and time . . . natural science is not Nature itself but a part of the relation between Man and Nature, and therefore is dependent on Man. The idealistic argument that certain ideas are *a priori* ideas, i.e., in particular come before all natural science, is here correct.[13]

As if the need for a revision in the classical space-time conception of the independent existence of physical entities had not been dictated by the structure of the quantum mechanical evidence rather than by either the constitution of our minds or the procurement of this evidence through the mediation of macro-apparatus.

What, then, is the most plausible ontology of physics consistent with the incorporation of quantum complementarity into the framework of a realistic epistemology?[14] Even an undiluted macrophysicalism will not do here. For there is a definite sense, as Max

[11] *Ibid.*, p. 22.

[12] *Ibid.*, p. 28.

[13] *Ibid.*

[14] The remarks which follow are predicated on the assumption that quantum mechanical complementarity is to be a permanent feature of microphysical theory for phenomena on the atomic scale of 10^{-8} to 10^{-10} cm. It is noteworthy that whereas Bohm, for example, hopes that data in the nuclear range below 10^{-13} cm. may turn out to justify the hypothesis of a deterministic substratum of hidden parameters (Bohm, *op. cit.*, pp. 121–126), L. Rosenfeld believes ("Strife about Complementarity," *Science Progress 41* (1953), p. 409) that "The problems raised by the field of nuclear force and the various particles associated with it demand for their elucidation new methods which will very probably mean a qualitative change in our conceptions comparable to that embodied in the passage from determinism to complementarity." This view is also shared by Born in his Nobel prize lecture, "Statistical Interpretation of Quantum Mechanics," *Science 122*, 1 (1955).

Born has pointed out in an incisive paper,[15] in which complementarist physics, while certainly denying the autonomy of the classical mechanical and electromagnetic attributes of state, does allow us to speak of the independent reality of atomic entities. Born's penetrating fundamental idea is to ground their independent existence on properties of theirs which, in contrast to the theoretically disjunctive ones, are *invariant* with respect to all particular observational contexts.[16] I now wish to develop his view as part of a comparison between the relativistic and quantum mechanical supplantation of the classically-conceived relation of observation to reality. This comparison will serve to explain, though *not* to justify, Einstein's adamant rejection of complementarity despite charges that his reasoning here is akin to that of Newtonian opponents of relativity theory.[17]

Although we shall find that a microphysically realistic interpretation of complementarity is warranted, this very interpretation will lead us to uphold the Copenhagen view that the temporal evolution of the mechanical properties of individual atomic systems is *irreducibly statistico-deterministic* rather than deterministic.

The theory of relativity did indeed stress what Bohr calls "the primordial importance of the observational problem,"[18] but it did so in regard to non-invariant metrical attributes. One can certainly *not* say that "the theory of relativity reminds us of the subjective character of *all* physical phenomena, a character which depends essentially upon the state of motion of the observer," as Bohr puts it carelessly.[19] For that theory regards such rudimentary events

[15] M. Born, "Physical Reality," *Phil. Quarterly 3*, 143 (1953); this paper is reprinted on pp. 151–163 of Born's *Physics in my Generation*, New York, 1956.

[16] The same thesis was put forward independently by L. Rosenfeld (*op. cit.*, pp. 405–406), but his unduly concise statement of it in terms of a purely *formal* invariance of the quantum mechanical equations with respect to the transformations expressing the passage from one mode of observation to another invited the inference by Bunge (*op. cit.*, pp. 9–11) that Rosenfeld's conception is a purely syntactical one without existential import.

[17] Cf. N. Bohr, "Discussion with Einstein" (hereafter cited as "DE") in P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist*, Evanston, 1949, p. 238, for a citation of P. Ehrenfest's barb, and W. Heisenberg, "The Development of the Interpretation of Quantum Theory," *op. cit.*, pp. 20–21, where the argumentation on behalf of a deterministic substratum for quantum theory is asserted to be logically identical with anti-relativist modes of disputation in favor of an undetectable absolute rest system.

[18] DE, p. 211.

[19] ATDN, p. 116, *my italics*. The unqualified reference to "all" phenomena is not confined to this one publication but is also found in the following other writings of Bohr's: DE, p. 224, and "Causality and Complementarity" (hereafter cited as "CC"), *Phil. of Science 4* (1937), p. 290.

as the collision of two particles or the coincidence of a pointer with a mark as *absolute:* the existence and qualitative constitution of such events are wholly independent of any system of coördinates and are thus logically prior to the specific metrical names assigned to them by any particular "observation post," names which do depend on the behavior of clocks and rods in the respective reference frames.[20] The fundamental involvement of the coördinate system arising from the dependence of the metric on the behavior of clocks and rods has the consequence that *some* of the properties of things and events which were Newtonianly thought to be *sui generis* and invariant turn out to be only *relational.* But even metrically, there are invariant properties, such as the *rest*-mass and *rest*-length of a body, which have the same value in any inertial system in which the body is brought to rest. The contrast with quantum theory centers on the fact that although the lengths, masses, and durations used by different reference systems to name events are only *relational* attributes, the *events* in question such as a particle's being at a certain point do *not* depend for their very occurrence on any *indivisible* coupling of the physical object with measuring devices under particular conditions of observation. Small wonder, therefore, that a thinker such as Einstein, whose entire cosmic outlook was molded by an absolutistic conception of events that continued to dominate his life-long field-theoretic endeavors, found himself unwilling to assent to the complementarist repudiation of the ontology integral to this conception. That it was prior ontology which induced him to be unyielding toward Bohr's epistemological dialectic is perfectly clear from Einstein's own declaration that his conception "begins" with the following thesis:[21]

> *There is such a thing as the "real state"* of a physical system that exists objectively, independently of any observation or measurement, and which, in principle, can be described by the concepts of physics. . . . Once you abandon this reality thesis . . . then it is a difficult matter to avoid solipsism.[22]

[20] For details concerning the sense in which this very summary statement should be understood in the context of the special and general relativity theories respectively, see A. Grünbaum, "Logical and Philosophical Foundations of the Special Theory of Relativity," *American Journal of Physics 23,* 450 (1955), and A. Grünbaum, "The Philosophical Retention of Absolute Space in Einstein's General Theory of Relativity," *The Philosophical Review 66,* 525 (1957).

[21] A. Einstein, "Einleitende Bemerkungen über Grundbegriffe," in A. George, ed., *Louis de Broglie,* Paris, 1953, p. 6.

[22] Almost identically the same considerations are mentioned by Louis de Broglie (in his *The Revolution in Physics,* New York, 1953, p. 235) as the inspiration for his own search for an interpretation of quantum mechanics allowing "a return to clear Cartesian conceptions, respecting the validity of space and time."

In the absence of a successful implementation of Einstein's hopes, it seems the better part of epistemological wisdom to content oneself with a realistic interpretation consonant with the complementarist denial of the absoluteness of point-events in micro-processes. Such an interpretation is offered by Born [23] via the principle that "the idea of invariant is the clue to a rational concept of reality" in the following way:

> We have to reflect about what we mean by a particle, for instance a photon, an electron, a meson, a nucleon in regard to the experimental evidence; . . . we find that these words signify definite invariants . . . characteristic of the entity. The main invariants are called charge, . . . rest-mass, spin, etc. . . . I maintain that we are justified in regarding these particles as real in a sense not essentially different from the usual meaning of the word.

Born shows here that the concept of "reality" or existence *does* apply to atomic objects but that the autonomous *attributes* of these independent existents are *not* conveyable by common-sense pictures. It is therefore an error to suppose, as Philipp Frank does, that the problem of the existential status of atomic objects is disposed of by the remark that

> We must remember, above all, that the word "reality" itself belongs to those "common-sense analogies," if it is applied beyond our common-sense language. . . . We invite trouble, if we ask the question, what are the "real" physical objects in subatomic physics. Are the particles "real" or are the de Broglie waves (described by the ψ-function) "real"? [24]

Frank overlooks that this untutored way of posing the reality problem in terms of a disjunction between equally illicit alternatives (see our footnote 7 above) is not at all the *only* way in which the question can be put. And his criticism of an initially misguided formulation of the reality problem can therefore hardly suffice to prove his macrophysicalistic claim that the domain of relevance of the concept of reality is confined to the macro-objects of classical physics.

Now that we know the independently existing attributes of atomic entities defined in Born's sense by the quantum mechanical invariants, it is perfectly clear that they do *not* constitute attributes of *individual* events *in space and time* which are the values of a set of state variables linked by deterministic laws. This result was to be expected from any philosophical interpretation compatible with complementarity, since, as Bohr has explained, "the renunciation of the ideal of causality in atomic physics which has been forced on us is founded logically only on our not being any longer in a position to speak of the autonomous behavior of a physical object." [25]

[23] M. Born, "Physical Reality," *op. cit.*, pp. 158 and 160.

[24] P. Frank, *Philosophy of Science*, pp. 241 and 244.

[25] N. Bohr, CC, p. 293; cf. also ATDN, pp. 4–5. On the other hand, Einstein, who regards the quantum mechanical ψ-function description as *incom-*

The conclusion that quantum mechanics must be held to be only a statistico-deterministic description of physical reality has been contested by Margenau [26] on the grounds that the ψ-function may be regarded as a causally unfolding state [27] and that the resistance to the latter interpretation springs from an unwarranted identification of causality with the particular states and laws of Newton's theory rather than with that theory's general formalism. But this view does not allow for the principal fact prompting the appeal to the states of classical physics as providing the prototype of causal relatedness: the conception that causality is a relation between attributes of *individual* events. It is therefore *not* the gratuitous retention of the particular states and entities of Newtonian theory which confers significance upon the claim that quantum mechanics has abandoned causality. Instead, the affirmation of that abandonment stresses that the successive ψ-values do not denote the attributes of individual occurrences but determine only the statistical distributions of such attributes in aggregates of occurrences. Thus, there is a very important sense in which the evolution of the ψ-function *cannot* serve to show that microphysical reality is deterministic.[28]

plete, is not persuaded by the complementarist rejection of an autonomous space-time behavior and hence concludes that "One can furthermore hardly avoid the suspicion that the statistical character of the theory is conditioned by the incompleteness of the description and that it has no bearing on things as they are" (Einstein, "Einleitende Bemerkungen über Grundbegriffe," *loc. cit.,* p. 10).

It is very interesting in this connection that on the basis of cybernetic considerations, Norbert Wiener has advanced arguments on behalf of a statistico-determinism which considers "causality as something of which there can be either more or less rather than as something which is either there or absent" (N. Wiener, *I am a Mathematician,* Garden City, 1956, p. 323).

[26] Cf. his Preface to E. Cassirer, *Determinism and Indeterminism in Modern Physics,* New Haven, 1956, pp. xvi–xvii.

[27] Margenau has dealt very suggestively (cf. his "Philosophical Problems Concerning the Meaning of Measurement in Physics," *Phil. of Science,* in press, and *The Nature of Physical Reality,* pp. 377–380) with the objection raised, among others, by Reichenbach (cf. "The Principle of Anomaly in Quantum Mechanics," *Dialectica 2* (1948), pp. 343–344) that there is no continuous causal linkage of ψ-states, since changes in the ψ-function arise from the occurrence of a measurement and are *not* then governed by Schrödinger's equation. Margenau suggests the abandonment of the customary interpretation of the measuring process as the reduction of a wave packet, which underlies this objection.

[28] Neither can the latter conclusion be established along the very different line of argument pursued by Freistadt (H. Freistadt, "The Crisis in Physics," *Science and Society 17* (1953), p. 232), who writes: "If there is *no* law that determines the exact time of disintegration of a given uranium nucleus, then who or what does?" For it will not do to ask a question deriving its *very significance* from the assumption of determinism, which is at issue here, and

3. The Philosophical Generalization of Complementarity

Bohr [29] and, under his influence, a host of other writers [30] have claimed applicability for the quantum mechanical logic of theoretical disjunctiveness of conjugates to the resolution of issues such as the free-will problem and physicalistic reduction versus autonomy of biology and psychology. Detailed criticisms of some of these attempted generalizations of complementarity have been offered by both E. Zilsel [31] and P. Frank.[32] I wish to supplement and qualify the critiques of these authors.

The central error that seems to me to vitiate a number of the generalizations of complementarity is the inference that if two properties are operationally incompatible, they are *eo ipso* theoretically disjunctive. We saw early in §2 that within quantum mechanics proper the issue of theoretical disjunctiveness arises for pairs of attributes named by conjugate parameters, because there is massive empirical evidence *apart from any operational incompatibilities* that precise conjugate properties *do not jointly exist* but can characterize a physical system only disjunctively. It is not surprising, therefore, that there are other contexts, in which, on the contrary, we do have inductive evidence favoring the simultaneous theoretical existence of pairs of attributes even though their members are operationally incompatible. In such cases, the observation of one such attribute merely prevents us from simultaneously registering the otherwise *well-defined* presence of the other. Thus, suppose that a man has a malignancy of the metatarsal bone whose existence can be observed only by a direct extensive pathological examination of that bone under surgical conditions, a procedure which is physically incompatible with the

then to point to the absence of an answer from the party *denying* this assumption as evidence for its truth.

[29] Bohr, "Light and Life" (hereafter cited as "LL"), *Nature 131* (1933), pp. 458–459; "Wirkungsquantum und Naturbeschreibung," *Naturwissenschaften 17* (1929), pp. 484–486; CC, pp. 289 and 294–296; DE, pp. 224 and 236; ATDN, pp. 15 and 20–23.

[30] M. Born, *Physics in my Generation*, pp. 231–232; J. A. Wheeler, *op. cit.*, pp. 372–376; D. Bohm, *Quantum Theory*, New York, 1951, pp. 168–172; J. R. Oppenheimer, *Science and the Common Understanding*, New York, 1954, *passim;* W. Heitler, "The Departure from Classical Thought in Modern Physics," in P. A. Schilpp, ed., *Albert Einstein: Philosopher-Scientist*, p. 197; and P. Jordan, "Quantenphysikalische Bemerkungen zur Biologie und Psychologie," *Erkenntnis 4*, 212 (1934) and *5*, 348 (1935).

[31] E. Zilsel, "P. Jordan's Versuch, den Vitalismus quantenmechanisch zu retten," *Erkenntnis 5*, 56 (1935); cf. also on pp. 178–184 the comments on Zilsel's paper by H. Reichenbach, O. Neurath, M. Schlick, and P. Frank.

[32] P. Frank, *Interpretations and Misinterpretations of Modern Physics*, Paris, 1938, pp. 1–33.

simultaneous observable use of the man's feet in walking. It is plain that no complementarist logic interdicts our ascribing to the man the attribute of harboring a pedal malignancy concurrently with his being seen walking around after his medical examination. In fact, once an impairment of walking sets in as a result of the malignancy, the latter, far from being complementary to the former, even *explains* the simultaneous presence of the former. How, then, can any conclusion of theoretical disjunctiveness possibly be drawn in situations, whether physical or not, for which it is *not* shown that they do posses *all* of the characteristic *logical* features of quantum complementarity? Yet precisely such reasoning is used in the complementarist treatment of the logical relations between free will and the causal determination of human decisions, as shown by the following recent statement by Bohr's distinguished collaborator J. A. Wheeler [33]:

> the far-reaching usefulness of this idea [of complementarity] in resolving what would otherwise appear to be logical paradoxes . . . [is illustrated by] the dilemma of free will and causality. There is no dilemma, Bohr stresses. The experimental conditions that permit a subject to exercise his free will are physically incompatible with the experimental conditions required for a causality analysis. . . . such an analysis makes sense only if one can measure all the electrochemical potentials in the brain, and make throughout a thoroughgoing biochemical assay. That type of dissection is perfectly conceivable in principle—but it is altogether inconsistent with the normal existence of the subject, and rules out the exercise of free will on his part. Free will and determinism, in other words, are complementary concepts, not contradictory; the two can never come into use at the same time! [34]

In the light of our analysis, this statement invites the following kind of comment: To begin with, I deny that there is any "dilemma of free will." [35] The complementarists evidently assume a *logical* incompatibility here between the two attributes in question and adduce the *operational* incompatibility described by Wheeler as a basis for inferring their theoretical disjunctiveness, which they then offer as the resolution of the supposed dilemma. Now, my criticism is that if there were a free-will problem, the comple-

[33] J. A. Wheeler, *op. cit.*, pp. 375–376.

[34] The range of applicability claimed for complementarity is epitomized by the inclusion of justice and love among the pairs of complementary concepts (cf. Wheeler, *op. cit.*, p. 374, and J. Whitt-Hansen, "Some Remarks on Philosophy in Denmark," *Phil. and Phen. Res. 12* (1952), pp. 390–391.

[35] The reasons I would give for this view are set forth in such publications as R. E. Hobart, "Free Will as Involving Determinism and Inconceivable without It," *Mind 43* (1934); Cassirer, *Determinism and Indeterminism in Modern Physics*, Ch. 13; and A. Grünbaum, "Causality and the Science of Human Behavior," in Feigl and Brodbeck, eds., *Readings in the Philosophy of Science.*

mentarist argument would not have the logical resources requisite to its solution. For despite the operational incompatibility cited by Wheeler, the two properties deemed theoretically disjunctive on the strength of that incompatibility *can* be *simultaneously* ascribed to man entirely legitimately, the causally evolving brain states being significantly present even when they are not observed, since, if I interpret Wheeler correctly, he does *not* assume them to require characterization in terms of quantum concepts. Although the complementarist solution is not logically viable, Bohr has the merit of repudiating the quantum mechanical proponent of the *liberum arbitrium indifferentiae* as follows[36]: "I should like to emphasize that the [complementarist] considerations referred to here differ entirely from all attempts at viewing new possibilities for a direct spiritual influence on material phenomena in the limitation set for the causal mode of description in the analysis of atomic phenomena."

Far from defending any sort of quantum mechanical *reduction* of the feeling of freedom, Bohr invokes microphysical considerations in an endeavor to show that the reduction of either psychology or biology to quantum physics is logically impossible. As he puts it:

> The only logical possibility of avoiding any contradiction between the formulation of the laws of physics and the [holistic or finalistic] concepts suitable for the description of the phenomena of life ought therefore to be sought . . . [in the fact that] every experimental arrangement suitable for following the behavior of the atoms constituting an organism in as exhaustive a way as implied by the possibilities of physical observation and definition would be incompatible with the maintaining of the life of the organism.[37]

And hence he concludes that "the existence of life must be considered as an elementary fact that cannot be explained, but must be taken as a starting point in biology."[38] It might *appear* that Bohr is guilty here of inferring theoretical disjunctiveness from mere operational incompatibility, and Philipp Frank has offered a criticism of this argument which is essentially equivalent to such a charge.[39] But Bohr's case seems to be stronger than this: in an article explaining the meaning of the generalized principle of complementarity, W. M. Elsasser has pointed out that the principles of quantum mechanics themselves entail that catastrophic

[36] Bohr, LL, p. 459. For useful criticisms of endeavors to ground the freedom of human action on "gaps" in the laws of nature, see P. Frank, *Philosophy of Science*, pp. 249–259.

[37] Bohr, CC, pp. 295–296.

[38] Bohr, LL, p. 458. Cf. also ATDN, pp. 22–23.

[39] P. Frank, *Interpretations and Misinterpretations of Modern Physics*, pp. 26–27.

macro-consequences would ensue from the simultaneous measurement of the positions of all the micro-constituents of a physical object (say an organism).[40] And this result, which restricts the feasibility of a detailed quantum mechanical account of (solid) macro-objects both inanimate and animate, seems to me to leave it an open question for future research whether Bohr could not show that biology (and hence psychology) cannot be reduced to a quantum mechanical description of the micro-constituents of organisms, since the assumption of the truth of the latter description may entail *logically* that the macro-system to which it applies *cannot* be a living system. If such a demonstration were successful, it would not suffice to show, however, that a reduction of biology to classical physics and chemistry is impossible. And hence Bohr's conclusion that life must be viewed as an elementary fact does seem unfounded even if quantum mechanics never requires basic revisions.

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II. THE PRESENT STATUS OF DETERMINISM IN PHYSICS

IN a debate which finds theoretical physicists of the first rank in radical disagreement, the philosopher's task is a hazardous one.[1] He must face the double handicap of his serious technical limitations and the guarantee of powerful adversaries, whichever conclusion he may espouse. In short, he is compelled to join a bloody and well-balanced battle with his hands tied behind his back. Thus my comments on the physical evidence today must be regarded as unrelated to that discretion which is the better part of valor. They are possibly valorous, but certainly indiscreet. Yet there is a purely philosophical task involved; for one cannot discuss

[40] W. M. Elsasser, "On Quantum Measurements and the Role of the Uncertainty Relations in Statistical Mechanics," *Physical Review 52*, 987 (1937). Elsasser explains (p. 989) that if "we measure simultaneously the positions of all . . . constituents . . . as a result of the measurement, the constituents of the system will have obtained very high average velocities . . . and the system will burst into a number of parts within a comparatively short time after the measurement." I am indebted to Dr. Paul Feyerabend for calling this article to my attention.

[1] This paper is based in part on work done with support from the National Science Foundation and the Hill Foundation, and I wish to express my gratitude to them as also to my colleagues at the 1957 sessions of the Minnesota Center for Philosophy of Science and the Colston Society Symposium in Bristol for their immensely valuable discussions of this topic.

determinism in physics as one discusses, e.g., radioactivity in petroleum deposits, since there is no commonly accepted philosophical analysis of determinism itself. So I shall begin with an examination of the doctrine itself, proceed to consider the physical evidence, and conclude with a brief attempt to relate my conclusions in these sections to the major consequential areas—the analysis of responsibility and the analysis of scientific explanation.

1. Analyses of Determinism

I shall begin with some rather elementary discussion, since most of the technical treatments start with what seems to me erroneous or odd definitions of determinism. The backbone of the deterministic thesis in the usual contemporary discussion might best be brought out by asking what it is intended to deny. I take it that it is at least intended to deny the existence of chance events, i.e., essentially random events. Usually, it is taken to deny further that any events are caused by others standing in certain space-time relations to them, such as temporal posteriority or unmediated distance. But there is first a more pressing problem to be attacked: what is meant by "an essentially random event." We have lately heard a great deal about the concept of randomness [2] and the main difficulty with it can be put readily enough. An event is said to be random, it seems, only if it is not part of a regular pattern of events. But there are an infinite number of patterns, i.e., precise generating functions for any finite sequence of events (even assuming each event can be described by a finite number of state-variables). Hence there can be no strictly random events in a finite sequence. This conclusion is much stronger than conclusions about the impossibility of *showing* or *proving* that an event is random.

Our first inclination is to say that one cannot regard the discovery of just *any* generating function as adequate grounds for asserting that the events are non-random. In the first place, *ad hoc* functions seem objectionable. We feel that a function devised after the 9,327th event and discarded after the 9,328th has no right to be called a pattern of the sequence. But how can we formulate this condition? We do not know in advance that the function will fail after one trial, so the condition cannot involve reference to that failure. Moreover, a function which works for 9,327 events would have worked at all earlier stages; so in a sense it has received very heavy and repeated confirmation. We can hardly hold it against a function that we happened not to think of it at the beginning of the sequence.

[2] G. Spencer Brown, *Probability and Scientific Inference*, Longmans, 1957.

The effect of these difficulties is to put us under pressure to relativize the notion of a chance event. In fact I am convinced that in applied statistics such a move is correct: one should talk about a finite sequence not as "random," but as *relatively* random with respect to (a) its length; (b) certain patterns, e.g., excess of single digits, ordered triples, etc.; (c) some standard of acceptable deviation from the mean chance expectation. But in dealing with the thesis of determinism, acceptance of such a relativization would surely amount to capitulation by the indeterminist. For if there is no categorical predicate "random" then it would seem there could never be any certainty about the identification of a chance event.

This conclusion is too strong, as it stands. But it does bring out an apparent asymmetry of the two positions which is important for understanding the "regulative principle" interpretation of determinism. It appears that determinism is not susceptible to disproof in the usual way of an empirical generalization or a mathematical generalization like Goldbach's hypothesis (that every even number is the sum of two prime numbers). For there is no such observation as "seeing that an event is purely random." But the procedure of disproving highly theoretical scientific statements such as the law of the conservation of mass-energy is by no means a matter of single observations. We may agree that both the determinist and the conservationist theses are logically equivalent to the denial of a single counter-example; but this is consistent with saying that (a) specific single observations can do no more than make them improbable, and (b) other considerations—logical or theoretical—may do at least as much. The determinist's thesis is naturally more general than the conservationist's—it is a thesis with consequences for nearly all physical inquiries, not only those involving quantitative techniques. But it is still an empirical thesis, in the usual interpretation. Moreover, in no interpretation is it really inviolable in a way different from the indeterminist's thesis; for just as the determinist can continue to say that a cause *may* still be found for a certain event, so the hardened indeterminist can continue to say that the best evidence for a particular causal ascription *may* turn out to be entirely fortuitous, a statistical artifact. Either position eventually becomes unreasonable or vacuous in the face of mounting counter-considerations, enumerative (e.g., the repeated failure to find causes) and abstract (e.g., the inconsistency of alternative theories, the philosophical similarity to the Aristotle-Galileo issue).

The most serious abstract consideration is Russell's version of the randomness point we have already raised. At every moment in

the world's history every parameter has a value. Define the Russell function as the set of correlations of that value with that time for each parameter. Then the world obeys the law expressed by the Russell function; and this holds for all possible worlds. Hence determinism is necessarily true.

Against this I wish to say that the concept of a *law* of nature is such as to exclude completely a wide range of mathematically possible *functions*—and the indeterminist's thesis asserts that some events are not wholly *law*-governed. Laws are not merely general propositions, nor are they general propositions in which no singular terms occur non-vacuously, nor are they those propositions which justify contrary-to-fact conditionals. Climatic laws, linguistic laws, and perhaps Kepler's laws provide exceptions to these conditions; but here I need a different point from the logic of laws. Take two unrelated laws L′ and L″, and form their conjunction $L = L' + L''$. Now, confirming a law entitles one to regard deductions from it as more likely; but confirming L via confirming L′ does not increase the probability of those deductions which are instantiations of L″. Hence L is not a law. *A fortiori,* one might argue, the Russell function is not a law.

But this appears invalid. L′ is equivalent to the set of its instances, and confirming any of these confirms L′: is not the same true of the Russell-function? In the first place L′ is *not* (and the Russell function *is*) logically equivalent to the set of its instances in the way L is equivalent to $L' + L''$, since its instances may well be infinite (in the usual misleading formalization they are always infinite). Secondly, the instances of L′ share a common formal structure with L′ in a way that is wholly untrue of L or the Russell-function and their instances. It is this feature which provides a good reason for the "rule of inference" way of talking about laws. The instances of laws are, and the instances of some universal propositions are not, examples of applying a rule. Conversely, it is only when they are instances of *a* law that observations confirm that law: instances of true prediction using a Russell-function do not directly confirm it, as a law; they at best confirm its continued use for making predictions *via a secondary hypothesis,* e.g., that it originated from an omnipotent God. This distinction is important and not easy to make in a short space. Perhaps I can best bring it out by asking you whether you would regard the existence of a successful prophet as proof that the world was deterministic. It seems to me clear that it is logically perfectly possible to have a precognitive agent in an indeterminist world, i.e., the mere fact that someone can predict successfully does not establish determinism. It is not even enough to require

a rational method for predicting, since we can quite rationally use the clairvoyant as an instrument. Determinism is only indirectly about predictability, and having a successful clairvoyant or the Russell-function only tells us indirectly about determinism. Clearly the clairvoyant's world is not deterministic unless his own predictions, i.e., utterances, are determined; and there are clearly variations of his world in which his predictions are governed only by the Russell-function, so we may confine our attentions to the latter. Now *if* we could have this function *in advance,* we should have some grounds for supposing that the events in the world were determined by the source from which the function came. If no one gave it to us, we would either never see it or, if we did, we would hypothesize that someone had produced it who knew how the world would be and from this we would usually infer that the world *had to be* that way. If, on the other hand, we found every possible Russell-function lying around, we would never bother to glance at them since we know one of them must be true. Only the secondary inferences make a Russell-function ever relevant to determinism. With a law in the usual sense, it is not necessary to give hypotheses about its origin in order to justify making an inference with it. When we believe in determinism we believe that laws exist which govern all events and this does not mean merely descriptions of the world (Russell-functions).

The further problem arises that there is no sense in which the Russell-function *could* be given in advance, unless it could be expressed in terms of ordinary laws (tautology); and if it cannot be given in advance, it doesn't even look tempting as cannon fodder for a ruthless determinist. This is another way of illustrating the dependence on secondary hypotheses of inferences using the Russell-function: the continued success of laws justifies their continued use, but something else has to be invoked to sustain belief in a function whose prediction for Tuesday has no relation to the one for Monday. All events occur at *some* time and in *some* place, and could be foreseen; but this doesn't show they're law-governed, just that they're events. Russell's move to determinism is via predictability, and that requires advance possession of an unsummarizable infinitely long function.[3]

[3] Suppose the world contains a ten-sided die only, and that this successively illuminates one of its faces, following the digits in the expansion of pi. Is it deterministic? I think we would want to say it was, even though the sense in which a *finite* summary could be given is obscure. But the Russell-function sophistry applies to *every* sequence of digits including those for which the nth digit is not recursively accessible *except* via the function; and *that* accessibility is illegal (i.e., not according to law).

I conclude that Philipp Frank is overly critical when he supposes the Russell argument shows that determinism is vague unless the exact form of the laws is given. There may be borderline cases—I shall raise some in a moment—but Russell-functions are nowhere near the borderline. To discover a law is not to discover the data, it is to discover a *pattern* in the data. Frank thinks that a possible exception could be made if reference was made, not to the exact form of laws, but to the existence of a Laplacean Intelligence. But the Laplacean account of determinism is radically unsound. Apart from the fact that it also involves the notion of law ("force," in his original formulation) and hence is no better off, it is at best a consequence of determinism and at worst entirely vacuous; for it is about the performance of a queer entity, an Intelligence, and determinism is not usually conceived to be a thesis about Intelligences. If the reply is made that "deterministic" is a dispositional property of universes, as "stable" is of certain physical systems, meaning that certain operations (examination and calculation by an Intelligence) produce certain results, the difficulty is not avoided. For such an analysis logically involves a law about Intelligences which is surely not intended. In fact, the concept of Intelligence is so vague that it is likely to be simply an anthropomorphic disguise for a condition of effective calculability on the laws, and hence returns us to the unvarnished account.

Furthermore, the Laplacean account would also rule out some worlds which we would regard as deterministic. Suppose, for example, that some variable in the world assumes a certain constant value for all even dates of the month and increases logarithmically during all odd dates, on each odd date picking up where it left off at midnight twenty-four hours before. There would of course be a discontinuous change in the variable every midnight, but a perfectly predictable one, of varying but bounded size. Should we not regard such behavior as deterministic? If so, then we must consider the possibility that all the state-variables act in this way. If they did, then Laplace's Intelligence would need *more* than the present state of the Universe and the laws governing it: we would also need to know the date and the hour. Now, one can rightly be puzzled how it could be determined that the whole Universe obeyed such a law when no time-measuring devices could possibly do so. I must reply that for a long time I fervently wished that the possibility could be dismissed as meaningless in this way, but I have now regretfully concluded that good scientific reasons could be given for its occurrence, and that modern cosmology certainly countenances similar possibilities. The ex-

tremely important result follows from this case that we cannot suppose "like effect follows like cause" to be an adequate analysis of determinism. For the state A at 11:55 p.m. on even dates is followed by the identical state A, whereas the state A five minutes later is followed by the very different state B, the initial state of the odd date. (It is tempting but entirely contrary to physical practice to regard the time-variable as a state-variable; and this is one of the crucial respects in which relativity has not made time into "just another dimension.")

As far as I can see, this kind of example invalidates a good deal of the discussion in the literature on the causal principle; I have in mind particularly Chapter 12 of Philipp Frank's recent and valuable book, *The Philosophy of Science.* Moreover, this cannot be saved by adding a condition of continuity, since the example given can readily be amended to provide continuous transitions from step to step. I described it in the more acute form partly because I wanted simultaneously to undermine the connection between determinism and continuity. It should be noted that even discontinous behavior such as that described does not violate the requirement, which we are not discussing, that a body cannot be in two places (states) at the same time. Nor, contrary to appearances, can I see any way in which it violates the theory of relativity; but even if it does so as described, it can easily be modified to conform.

In the light of these considerations we can comment quite briefly on the situation in physics.

2. The Present Physical Evidence

Once we see that the discovery of exact determination of an incomplete description of the world does not guarantee determinism, we are in a position to deal with one of the suggestions made about determinism in modern physics. If a roulette wheel always spins so as to keep the sum of two out of any three successive numbers exactly equal to 40, then we have a precise determination of an incomplete description; given the sequence 7, 23, 17, we do not know which number will occur next.

In quantum physics, we have a statistical function, the ψ-function, which characteristically obeys certain exact transformation rules, incorporated in the Schrödinger equation. It has been argued, by Nagel, that in an important sense this should be described as a form of determinism. Yet the ψ-function definitely does not enable one to make exact predictions about macroscopically observable events such as scintillations, particle tracks, and Geiger counter clicks. It is the theme of my argument in this

paper that determinism is not a matter of degree and is not relative to specific variables. Consequently I would have to regard the precise behavior of the ψ-function as of no more help to a determinist than the sum-function in the preceding example about the roulette wheel. The only vagueness in the deterministic thesis is the concept of event, and this is not at issue when a scintillation or a click is under consideration.

Now might one not argue that in the history of science we have commonly and correctly *abandoned* the search for certain methods of prediction, not succeeded with them? The attempt to predict fortunes by horoscopes would be a case in point. Might not quantum physics provide a further example? But there is a crucial difference between the two elements of this comparison. Astrology claimed to be able to predict certain phenomena P on the basis of a certain class of data D. We abandoned this claim indeed, but we still believe P is predictable on the basis of some other data D'. But quantum physics is able to give us very good reasons for supposing that P cannot be predicted on the basis of *any* D. We not only can't do it, but we can give strong arguments to show that it can never be done. So in *this* case, and not in the astrology case, we are tempted to abandon the thesis of *determinism,* not only the thesis of the ineffective theory; in this case, and not in the astrology case, we *cannot* count the failure of the theory to predict P as a defect of the theory, since the theory itself makes no claim to do so. It may well be that we should abandon the attempt to predict P (for conceptual, not practical reasons); but we must realize that in this case the cost is enormously greater.

Now one may ask how it can be that a theory can give reasons for supposing that certain predictions cannot be made. Is this not reminiscent of a theory about that most fascinating of all substances, ectoplasm, a theory which contains the assertion that it vanishes in any light strong enough to see it by? This surely contravenes scientific method.

This is a particularly interesting question and one that has been discussed in great technical detail. Let me indicate only briefly the lines of argument as I see them. It has commonly been asserted that there exists in quantum mechanics a categorical proof, the von Neumann proof, that no determinist theory is possible. Now if this proof starts from quantum mechanical premises, then it would clearly be possible to ignore it if one was prepared to consider alternative premises; and to consider a determinist theory would be to do just that. In fact, it *does* start from premises which are part of quantum mechanics; but two difficulties present themselves before we can cheerfully retain determinism and set

about discovering respectable deterministic theories. The first is that we have staggeringly strong evidence for the premises; strong enough, it seems to me, to immediately throw into doubt the deterministic thesis. The second difficulty is that alternative theories which begin from premises sufficiently close to those of quantum physics to be worth empirical consideration, and which nevertheless retain a deterministic appearance, have all failed to maintain that appearance upon closer scrutiny.

Let me make clear the difference between these difficulties. The evidence as it stands is such as to lend very great weight to the quantum-mechanical descriptions; and from these descriptions there necessarily follows (by the von Neumann proof) the indeterministic nature of the phenomena. That is, it would be logically inconsistent to accept these, the most plausible descriptions of sub-atomic processes, and maintain that there exist some underlying deterministic variables of which the quantum theory variables are manifestations (in somewhat the way that statistical measures of a population are manifestations of an underlying set of individual variables which could be entirely deterministic). The second difficulty involves no question of logical inconsistency within a theory; it is a matter of two lines of plausibility reasoning. For a theory of micro-events to be plausible at all in the face of present evidence, there are certain relationships which it must incorporate, either as approximations or as exact laws. (Nor is the evidence of a kind which may prove to have been mistaken; only the very grossest features of the very simplest experiments, e.g., in spectroscopy and photoelectricity, repeated many hundreds of times, are here involved.) For the theory to be furthermore deterministic and to provide interpretations of the many and various supposedly quantum phenomena, there are only a limited number of ways in which it can mathematically be developed; and there are very strong reasons for supposing that *none* of these withstand thorough elaboration. This second difficulty has been expounded most convincingly by Groenewold in several papers and discussions. To give a specific example, one might, following an early suggestion by Bohm, wish to talk of quantum mechanics as an extension of classical mechanics in the sense that it involves only the addition of a quantum potential to the classical potentials in the equations of motion. The equations appear thereby to preserve their deterministic character. But closer examination discloses that the new potential is unfortunately not susceptible to independent measurement; in fact, it already incorporates at least one of the state-variables which we hope to obtain from the equation in which it occurs. Hence it

is not significantly more deterministic than any "law" which "governs" a variable by saying "it is what it is"; and certainly it is not as satisfactory as the Russell function, apart from its greater simplicity. That there remains the logical possibility of devising a deterministic theory of the sub-quantum level is admitted when we refer to the above considerations as plausibility considerations; and Bohm and Vigier, amongst others, are following the guidance of Einstein himself in working on such theories. But I shall venture the extremely rash judgment that their activities strike me as very similar to that of the Cartesians, who, faced with Newton's proof that a theory of vortices could not account for Kepler's Laws, set about finding something near enough that would be good enough. Their honorable failure marked the introduction not merely of an alternative theory of celestial motion, but of an alternative method of physical explanation. Contact-action as an explanatory necessity was abandoned for (or at least fundamentally transformed into) field-action.

Just as the Cartesians could have "saved" their view by making the ether intangible, so it is possible to "save" classical physics in the quantum field by extraordinary assumptions, e.g., instantaneous transmission of causally efficacious influences from slits in screens to particles located at vast distances from the screen (Reichenbach). But not only are such assumptions wholly unjustified except as expedients for saving classical concepts, the exact set required shows signs of straightforward inconsistency, and has certainly never been formulated in a way that would salvage determinism. In short, this would not *save* determinism because (a) it would sacrifice truth and (b) possibly also consistency.

We now come to a wholly independent argument against the view that quantum physics is in a sense still deterministic. It is in fact *not* the case that even the ψ-function obeys deterministic laws. While this is true in many situations, including cases of strong interaction, it fails to be so in what are referred to as "measurements," the term being here used in a somewhat technical sense. Now it is not possible to support this statement fully in this paper. I owe my belief in its truth and my understanding of the arguments supporting it to Paul Feyerabend, and they represent a modification of his position in the paper "On the Quantum Theory of Measurement" in the 1957 Colston Society Symposium "Observation and Interpretation." One may mention that there are a number of ways in which it appears possible to save the deterministic character of the ψ-function, e.g., by dealing with the ψ-function of the observer-plus-observed system, but they all seem to me subject to irremediable flaws. The question of

how best to *interpret* these indeterministic "jumps" is an exceedingly interesting one; the Born interpretation of the measurement as causing the jump, the counter-paradox of the Schrödinger cat apparently killed by the "measurement" of looking to see whether a photon-operated gun has shot it or not, the use of a theorem of von Neumann's to show that one can consistently assert its earlier death, the paradoxes of Einstein-Podolski-Rosen and the standing-wave electron, the new attempts (Ludwig, Feyerabend) to represent the transition to a mixture (the "jump") as an approximating step, these moves and counter-moves provide a kicking example of conceptual reorientation of the greatest interest, discussed at some length by Grünbaum in the preceding paper. *But* the serious difficulties here are nearly all irrelevant to the determinism issue; nearly all the positions alluded to accept the indeterministic jump in one form or another and put forward a particular account of it. At least we can say there are grave objections to the view that quantum physics is derivatively deterministic *even* if we assume that this would be true if the ψ-function always obeyed the Schrödinger equation.

I should like to go into some other aspects of the determinism problem in physics, especially the question of retarded potentials (Feynmann) and the reversal of the causal order, and the kind of cosmological model which raises in acute form the time problem. But I wish to go on to some brief comments on two quite different points of great importance to a consideration of the present status of determinism, so I shall simply sum up my judgments on the purely physical side, for what they are worth, by saying that physics today seems heavily and rightly committed to a clearly indeterministic form. This is a conceptual change of a more fundamental kind than most—and perhaps all—that have gone before, because *the new physics offers no substitute for the answers it cannot give.* Instead of showing us that the questions were inappropriate (cf., Why does a body continue to move in a straight line under no forces?), or in need of further analysis before an answer could be given (cf., Why does man alone have the power of reason?), it claims simply that this world is not one in which the troublesome questions have any exact answer at all (When will this atom radiate?). For this claim it appears to me to have the best of reasons.

3. Human Freedom and Determinism in Physics

I hasten to say that I am not reopening (or reclosing) the question whether quantum uncertainty allows freedom of the will. I

presently regard this question as settled in the negative, not so much because this *could* not be the case, but because it does not seem to me the *correct* reason for believing in free will. If I have correctly described the status of determinism in physics, what can be said about determinism in psychology and hence (perhaps) in ethics?

It will not do at all to regard freedom (of the will) as the absence of external compulsion, simply because we do not regard the drug addict and the claustrophobe as having free choice with respect to certain alternatives which in no way involve external compulsion. What characterizes these people is the inevitability, the predeterminedness, of their actions in a situation which they may believe is a choice. (This inadequacy of the subjective criterion of choice is particularly clear with the post-hypnotic subject.) One might identify this inevitability with ineffectiveness of punishment in such cases; "free" means "responsible" which means "such that punishment (or reward) is appropriate, i.e., effective." This development collapses under the fire of such examples as the resistance hero who remains silent while being tortured to death; punishment does not alter his behavior, yet we wish to say he is fully responsible and *hence* heroic. The crucial difference here, it seems to me, is that we want to say the tortured man *could have spoken if he so desired* and *could so desire*, whereas at least one of these descriptions is not true of the neurotic and hypnotic subjects. But, the determinist may reply, it is not true that the underground hero could have desired to give away his secret; given his background and his circumstances his behavior was completely determined. Now, if physics is indeterministic, we must modify this claim. But we modify it in a way which does not directly assist the libertarian, for, as far as we know, undetermined events occur randomly with respect to choice situations; and only if a correlation could be demonstrated would it be at all possible to argue for a connection between physical indeterminism and moral freedom. Even then the Hobart argument must be met, viz., such events would not be the responsibility of the subject, simply *because* they were undetermined, i.e., out of character. But there is no great difficulty in meeting this argument by means of a distinction between wholly inexplicable acts and physically but not morally inexplicable acts; the argument is really much too strong to be true.

I revert to the determinists' original onslaught. This argument also proves too much. It involves a slide on the word "could" somewhat similar to that involved in arguing that no one person's decisions ever bring about a war because so many other conditions

are also necessary to bring it about; here it is "bring about" that suddenly slips through our grasp. The solutions of the two cases are similar; we decide whether the man in question could have decided otherwise, and could have done otherwise had he decided otherwise, by empirical tests which ultimately will involve treatment of the central nervous system as an information flow pattern.[4] To discover that the man could have decided otherwise is no more exciting or logically difficult than to discover that a train smash could have been averted (if the switchman had been awake) or that a certain forest fire could have wiped out a certain city (if the fire fighters had not reached it by plane). The compulsive is characterized by the fact that, in the extreme case, he could *not* have decided otherwise; he had no freedom of choice or will. Hence, even though a contrary decision would have produced a different action, we do not regard him (here) as wholly responsible. Similarly, in the conceivable but virtually unknown case of the man who is a prisoner in his own body, and to a lesser extent in cases of physical defect, we recognize that the decision was free but not causally efficacious, i.e., the man could not have *done* otherwise even if he had decided otherwise. A notion of partial responsibility is of course required for an adequate treatment of psychological determinism and moral blame; the cripple is *less* responsible for knocking over the Ming vase than the physically well man, the child from the slums is *less* to blame, at first, for moral transgressions. But the awful innocence of the determinist's denizens is a logical myth like the terrible helplessness of the fatalist's meditators. To say one's choice was free is to say that it could have been otherwise, but only if we insist on the same cause-same effect thesis does it necessarily follow that the choice would have been otherwise *only if* circumstances were different (cf. "Two identical radium atoms in identical circumstances can radiate at different times"). Of course, it is highly relevant to establish that different circumstances would have led to a different choice; but this is an evidential approach to, not a logical equivalent of, freedom of choice. The essential point about "could have done otherwise" is its context-dependence; there are no simple context-independent rules for deciding when it applies. But this in no way invalidates its utility and its meaningfulness in given circumstances, in physics or in law, and it is the cornerstone of morality. I conclude that determinism's demise in physics facilitates belief in but does not underwrite freedom of the will. I

[4] I am greatly indebted to Donald McKay for discussions of this issue, especially the communications theory aspects.

could have concluded otherwise just as this could have been a better paper and we all could have given more to charity last year. (But, I hasten to add, it would have been *unreasonable* of me to have concluded otherwise, whereas the other alternatives would probably *not* have been unreasonable!)

4. Deterministic and Probabilistic Explanation

In conclusion I want to say a very few words about the pay-off of the determinism issue, not in ethics, but in explaining. Granted that a very circumscribed set of requests for explanation in physics must be abandoned, are there any other effects of indeterminism on explanation?

The question is reminiscent of those which suggest profound results are bound to follow in every field if extra-sensory perception or even psychokinesis were established. For us traders in concepts, such discoveries are indeed important, and for certain limited areas in the relevant sciences also important; but relativity affected the timing of horse races about as much as quantum mechanics affects explanation (or, I would add, free will).

This is more easily seen if we realize that there are virtually no laws in physics which are thought to be literally true. Snell's Law, Hooke's Law, Newton's Laws, the Laws of Thermodynamics, Kepler's Laws, none of them are better than approximations and then only over a certain range of conditions. It is thus of negligible consequence for explanations involving such laws that *strict* determinism does not hold. All that is affected is our belief that we could explain—one day—every *deviation* from these laws, a merely programmatic change. Moreover, this is not even a very exciting programmatic change for the working scientist in, say, hydrodynamics or friction research, because there would never have been for him any possibility at all of fulfilling this program. He is unaffected by the change. More generally we can say that the search for causes is not affected (except in micro-physics), since the occasional undetermined events cannot be identified as such without exhaustive examination of possible causal accounts. In this sense—as a prescriptive rule for the researcher—determinism is not generally undermined. That there *may* not be a cause is no guarantee that there *is* not one.

The appeal of the deductive model of explanation is thus, I feel, largely illusory and in consequence little real damage is done to the actual procedure of explanation by the loss of deterministic laws. Indeed, as I have argued elsewhere, it seems to me apparent that in the explanation of much behavior and many historical

events we cannot regard the deductive model as even an ideal. The deterministic laws there, we can say with confidence, would be—if they existed—so complex that measuring the relevant variables would be far beyond our evidential resources. Consequently we adopt probabilistic explanation as the appropriate kind for those fields; since the fields are defined in terms of the kind of evidence available (history is *necessarily not* a special subsection of neurophysiology), it is self-contradictory to speak of deductive deterministic explanations as an ideal for history.

It seem to me clear that the same is generally—though not quite universally—true in physics. The nature of correspondence rules, bridge laws, models, the undefinability of theoretical constructs, the space-time continuum framework, all these and many other factors intrinsic to an understanding of physics *necessitate* the use of non-deductive probabilistic explanation. So that determinism in physics is not only dead but hardly mourned.

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THE NATURE OF ANALYSIS

I. ARE DISCOVERIES ABOUT THE USES OF WORDS EMPIRICAL?

SUPPOSE that we are sitting at dinner and discussing how a certain dance is danced. Let us suppose that the dance in question is one requiring the participation of a number of people—say one of the Scottish reels. And let us suppose that we have a dispute about what happens at a particular point in the dance; and that, in order to settle it, we decide to dance the dance ofter dinner and find out. We have to imagine that there is among us a sufficiency of people who know, or say they know, how to dance the dance—in the sense of "know" in which one may know how to do something without being able to *say* how it is done.

When the dance reaches the disputed point everybody may dance as they think the dance should go; or they may all agree to dance according to the way that one party to the dispute says it should go. Whichever of these two courses they adopt, there are several things which may, in theory, happen. The first is,

* To be presented at the meeting of the American Philosophical Association, Eastern Division, December 29, 1957.

chaos—people bumping into one another so that it becomes impossible, as we should say, for the dance to proceed. The second is that there is no chaos, but a dance is danced which, though unchaotic, is not the dance which they were trying to dance—not, for example, the dance called "the eightsome reel." The third possibility is that the dance proceeds correctly. The difficulty is to say how we tell these three eventualities from one another, and whether the difference is empirical. It may be thought that, whether empirical or not, the difference is obvious; but I do not find it so.

It might be denied that there is any empirical difference between the first eventuality (chaos) and the second (wrong dance). For, it might be said, we could have a dance which consisted in people bumping into one another. In Michael Tippett's opera *The Midsummer Marriage* the character called the He-Ancient is asked reproachfully by a modern why his dancers never dance a new dance; in reply, he says "I will show you a new dance" and immediately trips one of the dancers up, so that he falls on the ground and bruises himself. The implication of this manoeuvre is the Platonic one that innovations always lead to chaos—that there is only one right way of dancing (the one we have learnt from our elders and betters) and that all deviations from this are just wrong. But whether or not we accept this implication, the example perhaps shows that we *could* call *any* series of movements a dance. If, however, we started to call it a dance, we should have to stop calling it chaos. The terms "dance" and "chaos" mutually exclude one another; but although we cannot call any series of movements *both* chaos *and* dance, we can call any series of movements *either* chaos *or* dance; so the problem of distinguishing dance from chaos remains.

The first and the second eventualities (chaos and wrong dance) are alike in this, that, whether or not we can say that *any* series of movements is *a* dance, we cannot say that *any* series of movements is *the* dance (viz., the eightsome real) about the correct way of dancing which we were arguing. It might therefore be argued that, although it may be difficult to say what counts as *a* dance, and thus distinguish between the first and second eventualities, we can at least distinguish easily between either of them and the third (right dance). And so we can, *in theory;* for obviously both the wrong dance, and chaos or no dance at all, are distinct from the right dance. That is to say, the terms of my classification of things that might happen make it analytic to say that these three things that might happen are different things. But all distinctions are not empirical distinctions (for example,

evaluative distinctions are not); and the question is rather, How, empirically (if it is done empirically), do we tell, of these three logically distinct happenings, which has happened? And how, in particular, do we tell whether the third thing has happened (whether the dance has been danced correctly)?

Let us first consider one thing that might be said. It might be said: "The dance has been danced correctly if what has been danced is the dance *called* 'the eightsome reel.'" On this suggestion, all we have to know is how the expression "eightsome reel" is used; then we shall be able to recognize whether what has been danced *is* an eightsome reel. This seems to me to be true; but it will be obvious why I cannot rest content with this answer to the problem. For I am using the dance analogy in an attempt to elucidate the nature of the discovery called "discovering the use of words"; and therefore I obviously cannot, in solving the problems raised within the analogy, appeal to our knowledge of the use of the expression "eightsome reel." For this would not be in the least illuminating; the trouble is that we do not know whether knowing how the expression "eightsome reel" is used, is knowing something empirical. We shall therefore have to go a longer way round.

It may help if we ask, What does one have to assume if one is to be sure that they have danced the right dance? Let us first introduce some restrictions into our analogy in order to make the dance-situation more like the language-situation which it is intended to illustrate. Let us suppose that the dance is a traditional one which those of the company who can dance it have all learnt in their early years; let us suppose that they cannot remember the circumstances in which they learnt the dance; nothing of their early dancing-lessons remains in their memory except: how to dance the dance. And let us further suppose that there are no books that we can consult to see if they have correctly danced the dance—or, if there are books, that they are not authoritative.

What, then, in such a situation, do we have to rely on in order to be sure that we have really established correctly what is the right way to dance the eightsome reel? Suppose that someone is detailed to put down precisely what happens in the dance that the dancers actually dance—what movements they make when. We then look at his description of the dance and under certain conditions say, "Well then, *that* is how the eightsome reel is danced." But what are these conditions?

We have to rely first upon the accuracy of the observer. We have to be sure that he has correctly put down what actually hap-

pened in the dance. And to put down correctly what one actually sees happening is, it must be admitted, empirical observation and description. But what else do we have to rely on? We have to rely, it seems to me, on at least two other things. The first is that the dance which is being danced is indeed the eightsome reel; the second is that it is being danced right. These are not the same; for one may dance the eightsome reel but dance it wrong. For both these things we have to rely on the memory of the dancers; and, as I have said, it is not at all clear whether remembering something is making an empirical discovery.

The sort of situation which I have been describing is different from the sort of situation in which an anthropologist observes and describes the dances of a primitive tribe. This, it might be said, *is* an empirical enquiry. The anthropologist observes the behavior of the members of the tribe, and *he* selects for study certain parts of this behavior, namely, those parts which, by reason of certain similarities, *he* classifies as dances. And within the class of dances, *he* selects certain particular patterns of behavior and names them by names of particular dances—names which *he* (it may be arbitrarily or for purely mnemonic reasons) chooses. Here we have nothing outside the characteristic activities of the empirical scientist; we have the observation of similarities in the pattern of events, and the choosing of words to mark these similarities.

In the situation which I have been discussing, however, there are elements which there could not be in a purely anthropological enquiry. If a party of anthropologists sat down to dinner before starting their study of a particular dance, they could not fall into the sort of argument that I have imagined. Nor could they fall into it *after* starting the study of the dance. This sort of argument can only arise between people who, first of all, know how to dance the dance in question, but secondly are unable to say how it is danced. In the case of the anthropologists the first condition is not fulfilled. This difference between the two cases brings certain consequences with it. The anthropologists could not, as the people in my example do, know *what* dance it is that they are disputing about. In my example, the disputants know that what they are disputing about is how *the eightsome reel* is danced. They are able to say this, because they have learnt to dance a certain dance, and can still dance it, and know that if they dance it it will be distinctively different from a great many other dances which, perhaps, they can also dance. The anthropologists, on the other hand, have not learnt to dance the dance which they are going to see danced after dinner; and therefore, even if they have decided to *call* the dance that they are to see

danced "Dance no. 23" or "The lion dance" (supposing that they have been told that that is the name the tribe has for the dance), these names are for them as yet unattached to any disposition of theirs to recognize the dance when it is danced. The anthropologists will not be able to say, when a particular point in the dance is reached, "Yes, *that's* how it goes." They will just put down what happens and add it to their records. But the people in my example, when they say "eightsome reel," are not using an arbitrary symbol for *whatever* they are going to observe; the name "eightsome reel" has for them already a determinate meaning, though they cannot as yet say what this meaning is.

The second consequence is that, when my dancers have put down in words the way a dance is danced, the words that they put down will have a peculiar character. It will not be a correct description of their remarks to say that they have just put down how a particular set of dancers danced on a particular occasion; for what has been put down is not: how a particular set of dancers *did* dance on a particular occasion; but: how *the* eightsome reel *is* danced. It is implied that if *any* dancers dance like *this* they are dancing an eightsome reel correctly. Thus what has been put down has the character of universality—one of the two positive marks of the *a priori* noted by Kant (we have already seen that what has been put down has the negative characteristic which Kant mentioned, that of not being empirical). What about the other positive mark? Is what we have put down (if we are the dancers) *necessarily* true? Is it necessarily true that the eightsome reel is danced in the way that we have put down?

What we have put down is "The eightsome reel is danced in the following manner, viz. . . ." followed by a complete description of the steps and successive positions of the dancers. We may feel inclined to say that this statement is necessarily true. For, when we have danced the dance, and recognized it as an eightsome reel correctly danced, we may feel inclined to say that, if it had been danced differently, we *could* not have called it, correctly, an eightsome reel; and that, on the other hand, danced as it was, we could not have called it anything else. The statement which we have put down seems as necessary as the statement "A square is a rectangle with equal sides." I do not wish my meaning to be mistaken at this point. I am not maintaining that there is any temptation to say that the statement "The dance which we have just danced is an eightsome reel" is a necessary statement; for there is no more reason to call this necessary than there is in the case of any other singular statement of fact. The

statement which I am saying is necessary is "The eightsome reel is danced as follows, viz. . . ." followed by a complete description.

We may, then, feel inclined to say that this statement, since it has all the qualifications, is an *a priori* statement. But there is also a temptation to say that it is synthetic. For consider again for a moment the situation as it was before we began to dance. Then we already knew how to dance the eightsome reel, and thus for us the term "eightsome reel" had already a determinate meaning; and it would be plausible to say that since we knew the meaning of "eightsome reel" already before we started dancing, anything that we subsequently discovered could not be something attributable to the meaning of the term "eightsome reel"; and therefore that it could not be something analytic; and therefore that it must be something synthetic. Have we not, after all, *discovered* something about how the eightsome reel is danced?

There is thus a very strong temptation to say that the statement "The eightsome reel is danced in the following way, viz. . . ." followed by a description, is, when made by people in the situation which I have described, a synthetic *a priori* statement. Yet this, too, would be an odd result; for I could show, if I had time, that similar grounds could be given for considering all statements about how words are used as synthetic *a priori* statements. If, which I have seen no reason to believe, there is a class of synthetic *a priori* statements, it can hardly be as large as this. Probably what has to be done with the term "synthetic *a priori*" is to recognize that it has been used to cover a good many different kinds of statement, and that the reasons for applying it to them differ in the different cases. It is, in fact, an ambiguous label which does not even accurately distinguish a class of statements, let alone explain their character. What would explain this would be to understand the natures of the situations (as I said, not all of the same kind) in which we feel inclined to use the term; and this is what I am now trying, in one particular case, to do.

The peculiar characteristics of the situation which I have been discussing all arise from the fact (on which Professor Ryle has laid so much stress) that we can know something (e.g., how to dance the eightsome reel) without being able yet to say what we know. But the fact that it has these characteristics should not blind us to certain dangers. There is first the danger of thinking that it could not have been the case that the eightsome reel was danced in some quite different way. It is, of course, a contingent fact, arising out of historical causes with which I at any rate am unacquainted, that the dance called "the eightsome reel" has the form it has and not some other form. If it had some different

form, what my dancers would have learnt in their childhood would have been different, and what they would have learnt to call "the eightsome reel" would have been different too; yet the statement "The eightsome reel is danced in the following manner, etc.," would have had just the same characteristics as I have mentioned.

Next, there is the danger of thinking that if *anthropologists* were observing the dance, and had been told that the dance which they were to observe was called "the eightsome reel," *they,* in reporting their observations, would be making the same kind of statement—namely a non-empirical, universally necessary statement which at the same time was not analytic. They would not be making this sort of statement at all, but an ordinary empirical statement to the effect that the Scots have a dance which they dance in a certain manner and call "the eightsome reel."

Before I conclude this section of my paper and go on to describe more complicated kinds of dances which resemble talking even more closely, I have two remarks to make. The first is that unless *some* people knew how to dance dances, anthropologists could not observe empirically how dances are danced; and that therefore there could not be empirical discoveries about dances unless there were at least the possibility of the kind of non-empirical discovery that I have been characterizing. The situation is like that with regard to moral judgments; unless *some* people make genuine evaluative moral judgments, there is no possibility of other people making what have been called "inverted commas" moral judgments, or explicit or implicit descriptions of the moral judgments that the first set of people make.

The second remark is that I have nothing to say in this paper which sheds any direct light on the question (often confused with the one which I am discussing)—the question of the distinction between logic and philology. The features which I am trying to pick out are features as well of philological as of logical discoveries —and this makes them more, not less perplexing.

I will now draw attention to some differences between the comparatively simple dance-situation which I have been discussing so far and the language-situation which is the subject of this paper. Talking is an infinitely more complex activity than dancing. It is as if there were innumerable different kinds of step in dancing, and a dancer could choose at any moment (as is to a limited extent the case in ball-room dancing) to make any one of these steps. Talking is in this respect more like ball-room dancing than like reels—there is a variety of different things one can do, and if one's partner knows how to dance, she reacts appropriately; but

to do *some* things results in treading on one's partner's toes, or bumping into other couples and such further obstacles as there may be, however well she knows how to dance. Nevertheless there are a great many things which one can do; and not all of them are laid down as permissible in rules which have been accepted before we do them. There can be innovations in dancing and in speech—and *some* of the innovations are understood even though they are innovations.

Both dancing and talking can became forms of creative art. There are kinds of dancing and of talking in which the performer is bound by no rules except those which he cares to make up as he goes along. Some poetry is like this; and so is "creative tap dancing" (the title of a book which once came into my hands). The most creative artists, however, are constrained to talk or dance *solo*. It is not about these highest flights of talking and dancing that I wish to speak, but about those more humdrum activities which require the coöperation of more than one person, and in which, therefore, the other people involved have to know a good deal about what sort of thing to expect one to do, and what they are expected to do in answer. It is in this sense that I am speaking of "knowing how to dance" and "knowing how to talk."

What makes coöperation possible in both these activities is that the speaker or dancer should not do things which make the other people say "We don't know what to make of this." That is to say, he must not do things which cannot be easily related to the unformulated rules of speaking or dancing which everybody knows who has learnt to perform these activities. The fact that these rules are unformulated means that to learn to formulate them is to make some sort of discovery—a discovery which, as I have said, cannot be described without qualification as an empirical one. If a person in speaking or in dancing does something of which we say "We don't know what to make of this," there are only two ways of re-establishing that *rapport* between us which makes these coöperative activities possible: either he must explain to us what we *are* to make of what he has done; or else he must stop doing it and do something more orthodox. He must either teach us his new way of dancing or talking, or go on dancing or talking in our old way.

It might be said, dancing is not like talking, because dancing is a gratuitous activity, and talking a purposeful one; therefore there are things that can go wrong in talking that cannot go wrong in dancing—things which prevent the purposes of talking being realized. This I do not wish to deny; though the existence of this difference does not mean that there are not also the simi-

larities to which I have been drawing attention. And the difference is in any case not absolute. Some talking is gratuitous; and some dancing is purposeful. When dancing in a crowded ball-room we have at least the purpose of avoiding obstacles, human and inanimate. If we imagine these obstacles multiplied, so that our dance-floor becomes more like its analogue, that elusive entity which we call "the world," dancing becomes very like talking. And all dance-floors have at least a floor and boundaries of some kind; so no kind of dancing is *completely* gratuitous; all dancers have the purpose of not impinging painfully against whatever it is limits their dance-floor. And there are some markedly purposeful activities which, though not called dances, are like dances in the features to which I have drawn attention—for example, the pulling up of anchors (old style).

This analogy points to a way of thinking about our use of language which is a valuable corrective to the more orthodox representational view, in which "facts," "qualities," and other dubious entities flit like untrustworthy diplomats between language and the world. We do not need these intermediaries; there are just people in given situations trying to understand one another. Logic, in one of the many senses of that word, is learning to formulate the rules that enable us to make something of what people say. Its method is to identify and describe the various sorts of things that people say (the various dances and their steps) such as predication, conjunction, disjunction, negation, counting, adding, promising, commanding, commending,—need I ever stop? In doing this it has to rely on our knowledge, as yet unformulated, of how to do these things—things of which we may not even know the names, and which indeed may not *have* names till the logician invents them; but which are, nevertheless, distinct and waiting to be given names. Since this knowledge is knowledge of something that we have learnt, it has, as I have said, many of the characteristics of memory—though it would be incorrect, strictly speaking, to say that we *remember* how to use a certain word; Plato's word "recall" (*ἀναμιμνήσκεσθαι*) is perhaps more apt.[1] As in the case of memory, however, we know, without being, in many cases, able to give further evidence, that we have got it right. And often the only test we can perform is: trying it out again. In most cases there comes a point at which we are satisfied that we have

[1] In the full paper of which this is a sawn-off version, I drew attention to certain analogies between my own account of this difficult problem, and Plato's solution, in *Meno* 80 d ff., of a similar problem, viz., the paradox of analysis. I should like to think that my account of the matter is a demythologized version of Plato's.

got the thing right (in the case of speaking, that we have formulated correctly what we know). Of course, the fact that we are satisfied does not show that we are not wrong; but if once satisfied, we remain satisfied until we discover, or are shown, some cause for dissatisfaction.

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II. DO WE DISCOVER OUR USES OF WORDS?

MR. Hare builds his paper on analogy between physical skills and the uses of language. People with physical skills may be able to perform certain activities without being able to say what they do or what rule they follow. The contrast between what one does and how one describes what one does is perfectly clear and no one, to take Mr. Hare's example, would confuse dancing the eightsome reel with describing the dance. This distinction is no longer clear, however, when one comes to language and it is by no means apparent that one can always know how to use a word without being able to say how it is used. To take a simple example, a person would hardly be said to know how to use the term "father" unless he could decide on logical grounds the truth of the sentence, "A father is a parent," "A father is a male," and "All male parents are fathers." Yet one way of giving at least a rough formulation of the rules for using the term "father" would be to say "Use 'father' synonymously with 'male parent.'" Thus knowing how to use the term involves knowing some of the rules of use and knowing them not merely in the sense of being able to follow them, but in the sense of being able to formulate them. More generally terms have, among other uses, uses in statements which are logically true, and among these are analyses and definitions.

It may be the case, therefore, that one cannot know how to use a term without also knowing how to analyze it. Certainly one cannot know all the uses of a term without knowing its analysis, but "knowing how to use a term" is a loose expression and probably does not require knowing all the uses. I would not want to insist that what has been said about "father" applies equally to every other term, and while I would think that one could not know how to use the term "father" without knowing its definitions, there would be other terms such as "space" and "substance" where one could. At most a partial set of rules would be required and perhaps not even that. My point, however, is not that knowing the use of a term always requires knowing its analysis, but rather that the distinction between knowing how to act and saying

what rules one follows is not clear in language. Language, in fact, is the one place where it is not clear, and this means that analogies to other activities have their least value here. Insofar as they suggest that the clear distinction can be made within language, they are definitely misleading.

In spite of this difficulty, the analogy seems attractive and perhaps it could be revived by a clearer specification of what it is to use a term. Uses of language are multifarious and perhaps some division among them may enable us to distinguish the uses which are like doing or acting from those which are like speaking about actions. If this is the case, speaking, or at least some sorts of speaking, may be put on all fours with dancing; and a discussion such as Mr. Hare's would once more become illuminating.

One easy suggestion as a start is to equate using a term to applying it. We use a word when we point to or otherwise indicate something and say that the word applies. "This is a chair," "That is red" become paradigms of use. No one could doubt that this is a use of language and an important one, one which children learn perhaps as the first. It has the advantage, moreover, of being easily separable from anything like the analysis of the term. It would be almost impossible to apply a term correctly without following the rules for its use, but at the same time, one need not be able to formulate the rules in order to follow them. Thus, we may say along the lines Mr. Hare at least suggests, to analyze a term is to formulate the rules we follow in applying it.

As far as it goes, this suggestion is not bad, but a difficulty becomes apparent at once: Not all terms can be applied in the sense of our paradigms. In general, adjectives and common nouns can be used as indicated, but verbs, prepositions, and adverbs cannot. One cannot point and say "This is between" in the sense in which one can say "This is a chair." A number of possibilities are open. A well-known one is to invent an object to be pointed at or indicated, in this case perhaps the ordered triad of objects, one of which is between the other two. We may then say that "between" is applied to this triad in the sense in which "chair" is applied to the chair. Some such procedure will work in a number of cases and of course is enshrined in Russell's logic. It is by no means clear that all terms can be handled in this manner, and adverbs would particularly be troublesome. To say of an action that it was done slowly would involve the further artificiality of treating "slowly" as an adjective applying to the ordered pair involved.

If this procedure is considered too artifical, there is an alternative. We may relax the narrow requirement of being able to

apply a term in the sense of pointing to what the term is predicated of and we may then say that, given a situation and a term, we can use the term of that situation if we know how to decide what sentences containing the term are true of the situation. This is not intended as a final statement but merely as an indication of the sort of statement which is required. One would, for example, not be expected to know how to decide the truth of all sentences but only of relatively short sentences—say those which can be uttered at a normal speed in five minutes or less. Again, the requirement of knowing how to decide may appear to be too strong, since one knows the truth of some scientific statements without knowing how their truth is decided. Again, perhaps the whole discussion should be restated in terms of probabilty rather than truth—this raises another host of questions. Whatever the emendations, however, a formula along the lines of the one given will serve merely to indicate what it is to know how to use a term of a situation. To know how to use a term in general would have to be explained as knowing how to use it in many, or most, or common situations.

It may be thought that I am leaving this account in entirely too vague a state and, certainly, much more discussion would be necessary before it became explicit; but my purpose at the moment is not to work a sense of "using a term" which will exclude knowing the analysis of the term, but rather to indicate some of the complexity of the problem. I have barely indicated two ways of solving it, one by restricting use of denotation and inventing denotata as needed. Alternatively, one may speak in terms of situations in which the term may be used. I am sure there are other possibilities, though I very much doubt that they would be simpler than those mentioned. The question I wish to raise is this: When we explicate "use of a term" by means of denotata or situations or in some other way, how much help are analogies to dancing, pulling up anchors, and the like? It would seem that if we are to avoid being misled, "knowing how to use a term" must be hedged with so many and such complete restrictions that the parallel to knowing how to dance is unimportant. The discussion must be carried on in terms of logic and logic alone. I am forced, therefore, to reject Mr. Hare's analogy as being misleading at its face value and useless when the complexity of the problem is seen.

We have noticed some general reasons for suspecting analogies between the use of language and any other activity. There is, however, a more particular objection to the form in which Mr. Hare develops the analogy, a difficulty which he himself half

recognizes. His first analogy to speaking is doing the eightsome reel, a dance with which I am unacquainted but which, I gather, prescribes a rigidly fixed routine. In such a case, if one knows the dance, one can formulate its rules simply by noticing what one does as he dances it. Such a dance might form an analogue to reciting, but it does not reflect the variety of possibilities allowable in carrying on a conversation. Mr. Hare, of course, realizes this and so shifts his analogy to ball-room dancing which, like conversation, allows a number of alternatives at various junctures. What he does not recognize, however, or at least does not recognize explicitly, is that one cannot discover the rules of a ball-room dance simply by doing it. At most one can find one way of doing the dance and admittedly there are a number of others. The only way in which one could comprehend all of these possibilities would be to consider all of the alternatives at every step of the dance. This might be possible, but it is no longer the case that simply performing a physical action enables us to formulate rules; rather, the whole investigation moves into the mental sphere and it is by considering possibilities that we arrive at our rules. I do not doubt, of course, that a consideration of possible cases is an important aspect of the working out of rules, but this is a far less startling claim than what I, at least, understood Mr. Hare to be implying—that we could do philosophical analysis simply by listening to ourselves talk.

With all these objections to Mr. Hare's analogy, there still remains the central problem to which he addresses himself. We use words in a variety of ways on a variety of occasions. This, of course, is an empirical truth. Other people use words in the same way or very nearly the same way as we do. This, again, is an empirical truth, and it is also an empirical truth that just these people rather than others use words in this way. We cannot always or even in general formulate complete rules for our use of terms. This, once more, is an empirical truth. Philosophers engage in formulating these rules. Again, an empirical truth. What they do in formulating these rules, however, according to Mr. Hare is not to discover one more empirical truth, but rather to make a discovery which one might well call a synthetic *a priori* truth if it were not for the ambiguities of the term "synthetic *a priori*."

In opposition to Mr. Hare, I shall contend that what is involved is primarily a matter of reaching a decision. There may be discovery incidental to the decision, but if so it is discovery of the ordinary empirical sort. The decision is not, of course, arbitrary—it is made for good and often self-conscious reasons; but it is none

the less to be thought of as a decision to act rather than a discovery of truth.

We may begin by considering a situation like that of Mr. Hare's anthropologist watching a dance, a situation in which I am interested in another person's use of language. It may be his use of a term or of a sentence or of a type of grammatical construction, it does not matter; for the sake of the illustration let it be a term. Boswell-like, I would listen to his conversation and keep a record of his usage. To get more material I might introduce topics of conversation which would involve the term. I might even ask hypothetical questions as to how he would use the term under conditions which did not in fact obtain. Probably I would attach less weight to these latter than to the actual employment of the term but still they would be taken into account. After obtaining a large sample of this sort I would attempt to codify the usage, to formulate rules to which his use of language conformed. There are two aspects of the conclusions which might be reached which are worth pointing out.

First, strictly speaking, I would have found rules according to which he had used the term during a given time-interval. The record would probably not even be complete, but certainly it would be no more than a record of use during a certain span. Since "How X uses a term" presumably means something different from "How X used the term between times t_1 and t_2," I have not found how X uses the term but must merely make a more or less probable inference from the rules he followed during the observation period. The probability would, as usual, depend on the completeness of the record, the frequency with which the term was used, the length of the time-span, and the occurrence of any unusual incidents which would upset the tenor of X's life.

Second, I have not found the only set of rules compatible with X's utterances. Presumably there would be others, perhaps more complicated, which, had he been following them, would have led X to use the term in the same way. New situations would help discriminate between alternative sets of rules, but no finite experience could give rise to a unique set of rules. Thus, if one were investigating someone's use of the term "mammal" before the discovery of the platypus, it would have been equally compatible with the rule "Apply 'mammal' to all haematothermal viviparous lactiferous quadrupeds" and "Apply 'mammal' to all haematothermal lactiferous quadrupeds." Even today one cannot tell from other people's use of the term whether the requirement of being lactiferous is necessary; and even if asked whether they would call a warm-blooded quadruped which didn't give milk a

mammal, most people would not know. The set of rules derived from such observations is clearly not unique.

There is no doubt, then, that the set of rules arrived at as constituting another person's use of a term is at best an inductive generalization. The set of rules formulating observations are only one formulation among many, and the transition from past use so formulated to use in general represents a typical inductive inference. These conclusions are hardly controversial and are interesting not for themselves, but for comparison in the case in which I am trying to formulate rules of my own usage.

In this case, as well as the other, I may remember or invoke other evidence of instances of my use of the term. I may as before and in a quite impersonal way formulate rules which codify my past uses of the term. As before, these rules will not be unique, but there is an advantage in one's own case. Given some pair of rules, both of which conform with one's actual past usage, but which differ in their prescription for some case which has not arisen, I can at once ask myself what I would do in that case. It is not sure that I would get any important information. I may be unable to make up my mind. I may favor one rule over the other and I may tell myself that I would decide so as to confirm that rule—which would hardly be evidence in the ordinary sense. Still I may come up with something useful and so might be able to eliminate some alternatives. This could not be expected, however, to go so far as to give one a unique set of rules. There would be too many situations in which one would just not be sure how he would use the word. Here there is no difference in principle between formulating rules for one's own use and those of other people.

When it comes to the step of going from one's past use to his use in general, however, the situation is more complex. As before, one could distinguish between "my use of a term" and "how I have used a term," and I take it that the difference would be of the following sort: If I have used a term in a certain way, but no longer intend to use it in that way, the way does not constitute my use of the term. Neither does the way in which I intend to use it. That way becomes my use only after I have used it for a little while at least. Thus having used a term in a certain way is a necessary, but not sufficient, condition of using it in that way. I do not insist on this way of speaking, though it seems quite natural to me; and my point can be made equally well so long as having used a term in a certain way is not a sufficient condition of that being my use of it.

Granted this last claim we are, as in the case of another person,

confronted with the transition between "I have used a term according to a certain rule" to "I shall continue to use it according to that rule." If this were a simple induction as in the case of another person, one could agree that analysis represents a discovery, though, of course, it would be an empirical discovery. One does not, however, predict his own conduct in the same way that he predicts that of another person, and the reasons for this are simple and well-known.

Every prediction, as distinguished from a mere guess, is based on certain evidence and assumes that certain factors will be decisive in the occurrence or non-occurrence of the event predicted. If some new factor enters the situation, the result of the prediction is rendered uncertain; and if one knows that a new factor will enter, he must have less confidence in his prediction. Suppose I predict a friend's conduct, basing my predictions on my knowledge of his habits, his interests, and the situation in which he will find himself. Suppose I also tell him what I have predicted about him. The fact that I have told him, along with any desires he may have to vindicate my judgment or else perhaps to show that he is an inscrutable person, will certainly influence his conduct and render my prediction doubtful. Of course, if I know him well enough I may try to predict how he will react to this information and so predict his conduct on the basis of his knowing the first prediction. But if I tell him of this second prediction it will be vitiated in the same manner as the first.

The point of this discussion is that if I am predicting my own behavior I cannot conceal the prediction from myself and there is the same sort of interference as when one tells another what has been predicted concerning him. Any prediction I make about myself is vitiated by the fact that I make it. This is not to say that one cannot predict one's own future. One can, insofar as the future does not depend upon oneself but upon external circumstances. One can also predict some events of his own action if these are minor or far distant so that the prediction is likely to be forgotten and does not interfere. This is recognized in daily life. While I may predict the weather or coming political events of your conduct, I do not predict at what time I shall have lunch or go to bed. These are questions which I simply decide.

It is the same, I believe, with the formulation of rules governing how I use terms. If I am interested in making the rules explicit (not everyone is, of course), when I notice that alternative sets of rules would account for my usage, I either decide between them or else I decide not to decide. I may even, for various reasons, decide to follow somewhat different rules than previously,

as a reader of Russell's logic might decide to use the definite article in a somewhat different manner than he had before. Thus, it seems we do not formulate our own use of terms inductively as we formulate another person's, nor do we invoke any synthetic *a priori* principles to get at rules of use. We simply decide them. Mr. Hare is right that there is something new involved, but this is not a synthetic statement but merely a decision.

To say that an analysis represents a decision is not of course to claim that it is an arbitrary decision. Philosophers have reasons for these decisions as well as any others, and it may be well to examine the chief sorts.

(1) Philosophers like other people wish to be understood. They do not, therefore, depart far from their own and established usage. This is to say that an excellent reason for deciding to follow a certain rule for the use of a term is the fact that one has spoken in accordance with the rule and that other people do so as well. Where usage is not fixed and where there are doubts, there is greater freedom to set up an arbitrary rule.

(2) Philosophers find certain ideas clearer than others. It is not always evident why that is the case and there seem to be fashions in the matter. Idealists of the nineteenth century seem to have found mental terms clearer than physical and tried to analyze physical terms by means of them. Today the opposite tendency seems stronger. I do not know of any way of deciding who is right or, for that matter, how one can escape his own findings; perhaps one must start with whatever he finds clear. Mr. Hare, for example, has indicated his mistrust for any analysis in terms of "facts." He seems willing, however, in constructing a surrogate for "ought," to use such notions as "please" and "part of a sentence common to indicative and imperative moods." I cannot see that there is a marked advantage. My purpose is not, however, to dispute Mr. Hare's choice of terms to use in analysis, but rather to point out that one does make such a choice and the analysis is considered valuable only if it explains by means of terms already considered clear. Many logicians, for example, would rejoice in an analysis of contrary-to-fact conditionals in terms of a truth-value logic, but not if intensional concepts were brought in.

Another way of making the same point is to say that analysis is essentially reductionistic and what it reduces to is something antecedently clear. I do not mean to disparage it by calling it reductionistic, but merely to point out that it provides a means of eliminating a term and replacing it with clearer terms.

I do not claim that these are the only considerations which lead

to an adoption of a certain rule of use but they are enough to show that the decision is not arbitrary. What we have, then, is no discovery involving a new kind of knowledge, but merely making up one's mind to follow a certain rule. Though this, I believe, is what the philosopher does, this is not always the way in which he presents his conclusions. More often he will offer them as proposals, as recommendations of rules to be followed. Sometimes, even, crediting the reader with intentions of conformity to usage and desire for clarity, he will say that the analysis represents what the reader really means.

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III. SOME REMARKS ON PHILOSOPHICAL ANALYSIS

I PROPOSE to divide my contribution into two parts. Part I will contain some criticisms of Mr. Hare's paper and an attempt to provide what seems to me a simpler and more adequate solution of the problem which he poses. In Part II I make use of this result to discuss the limits of a philosophical method consisting in the discovery of the correct use of words. I do this by drawing attention on the one hand to certain crucial philosophical questions which concern the scope of the method and which cannot be answered by means of it; and on the other hand, to the fact that a great deal of legitimate philosophical activity going under the name of philosophical analysis is not, or not merely, concerned with the discovery of the use of words.

Although my Part II will only touch upon Mr. Hare's paper incidentally, the two are not wholly unconnected: Hare's use of Kantian terminology and his reference to Platonic doctrine are, it seems to me, all too easily taken as encouraging the view that philosophy, apart perhaps from alien accretions of myth and sheer nonsense, has always—to a very large extent or wholly—been the discovery of the use of words. This view, which I believe to be mistaken, makes it desirable that one should indicate the narrowness of a philosophy so conceived.

I

1. Hare's account

It has for some time been usual to take very seriously the analogy between speakers of a language and the players of a social game. While keeping this comparison in mind, I shall nevertheless in the main follow Hare's helpful analogy between the speaking of a language and the art of dancing. In particular, I shall use his

illustration of a group of people who all know how to dance the eightsome reel and who try to settle a dispute as to how it is danced by actually dancing it.

First, a minor objection: Hare distinguishes three possible results, mutually exclusive and jointly exhaustive, from this experiment: (a) chaos, (b) an incorrect performance, and (c) a correct performance. The dancers may, however, not be able to make such exact distinctions as these and may quite properly regard certain performances as border-line cases—whether between chaos and correct performance or between a correct performance and an incorrect one. For their decision about correctness or otherwise does turn, among other things, on whether a particular performance possessed certain empirical characteristics; and of empirical characteristics there can always be not only positive and negative but also border-line instances.

A more serious difficulty is raised by the conditions which according to Hare must be fulfilled if the disputants are to be sure that they have by their experiment established the correct way of dancing the eightsome reel. They must, he says, apart from accurate observation and description, rely on their memory "that the dance which is being danced is indeed the eightsome reel" and "that it is being danced right." He regards these conditions as separate in that "one may dance the eightsome reel and not dance it right."

Yet the remembering and, more generally, the knowing how a certain dance is performed implies being able, in some cases at least, to distinguish between correct and incorrect performances of it; and to have this ability is, at any rate to some extent, to know how it is danced *correctly*. A person who completely lost the ability to distinguish between correct and incorrect performances of the eightsome reel would *ipso facto* completely lose the ability to distinguish between dances which are and those which are not performances of it. In this respect the relation between 'eightsome reel' and '*correctly danced* eightsome reel' ('chess' and '*correctly played* chess,' etc.) differs from that between, e.g., 'flower' and 'blue flower.' A person who, say by becoming color blind, completely lost the ability to distinguish between flowers which are and flowers which are not blue, would not *ipso facto* lose the ability to distinguish betwen physical objects which are and those which are not flowers. The relation in the latter case, but not in the former, is that of genus to species—as will, I hope, become clearer in the sequel.

Having made the distinctions which I have been criticizing, Hare addresses his main problem, namely, the clarification of the

logical status of statements to the effect, e.g., "that the eightsome reel is danced in the following manner, viz., ..." One may wonder why he records so strong an inclination to regard any such statement as a Kantian synthetic *a priori* proposition—a term which he considers as too ambiguous to be useful.

His own account is tantalizingly brief. It amounts to no more than saying that the peculiar characteristics of the situation (which one is tempted to explain in terms of synthetic *a priori* propositions) "all arise from the fact . . . that we can know something . . . without being able to say what we know." Mr. Hare mentions Plato and Professor Ryle in this connection. But I am even more forcefully reminded of Jakob Friedrich Fries's account of synthetic *a priori* propositions, which he regards as merely articulating or repeating in propositional form an originally obscure pre-propositional knowledge. This pre-propositional knowledge Fries considers to be common to all rational beings and not merely to the possessors of a particular know-how.[1]

Now, that one may know all kinds of objects, facts, and skills without being able to formulate one's knowledge in propositions seems to me both true and philosophically important. Yet the logical status of the formulated propositions will depend not on their being formulations of something known before, but rather on what this was, that was so known. Thus a person who formulates his pre-propositional knowledge of having collided with a bicycle formulates it in an empirical proposition. Again, *if*, as Kant held, we have an *a priori* intuition of space, then the axioms and theorems of the geometry of this space are synthetic *a priori*—and so on. Hare merely says, about the knowledge which his dancers formulate, that it is originally pre-propositional and a knowing-how. This seems to me insufficient.

2. An alternative account

The problem of Hare's dancers is to formulate their pre-propositional knowledge. They do so, I submit, by stating a set of rules. A rule is a proposition which is neither true nor false, but which stands to other such propositions in relations of logical incompatibility and in other logical relations, and which human beings can satisfy (conform to) or violate by their behavior. In its context, moreover, the statement of the rules implies that the dancers have, as I shall say, "adopted" them. A person has adopted a rule if he has the intention to conform to it in certain

[1] For an exposition and criticism of this doctrine, see, e.g., Paul Bernays "Über die Fries'sche Annahme einer Wiederbeobachtung der unmittelbaren Erkenntnis," in *Leonard Nelson zum Gedächtnis*, Göttingen, 1953.

specifiable circumstances. This intention may be present in a person who is not as yet, and perhaps never will be, capable of formulating the relevant rule. Its presence may manifest itself in various ways; e.g., in the fact that the person is able to distinguish actions which conform to the rule from those which violate it, calling the former actions "correct" and the latter "incorrect," expressing the distinction in cognate terms or expressing it nonverbally by gestures—smiles and frowns and the like.

My view, that Hare's dancers and others engaged on similar tasks of recollection are formulating rules, may be supported by the following considerations. According to H. A. Thurston [2] the eightsome reel was indeed "worked up by the late Earl of Dunmore and several friends from their recollection. . . ." The result is formulated thus: "The dance starts with a forty-bar introduction: *hands round and back* (8 bars); *double hands across and back* (8 bars);. . . ." The italicized words are clearly not used to describe a fact or event, nor any particular piece of actual behavior; they indicate the behavior which *would be correct* if it did happen. They express not true or false propositions but rules which can be satisfied or violated.

We may oppose the distinction made by the dancers between correct and incorrect performances before the rules are formulated as inarticulate or pre-propositional to their later articulate or popositional distinction in accordance with formulated rules. Using this terminology we can say that they arrive at the formulation of the rules by comparing their inarticulate distinctions with successive attempts at formulation until they are satisfied that the inarticulate distinctions and the articulate coincide. This process of establishing the rules which one has adopted is quite different from establishing propositions which are true or false. (The rules, of course, must be sharply distinguished from the empirical proposition that somebody has adopted them.)

In view of the obvious parallelism between social dances and social games it is relevant to consult von Neumann and Morgenstern's *Theory of Games*,[3] for an explanation of its central notion of a game. As one would expect, they define a game as a set of rules and distinguish sharply between a game and a play of the game, which is a performance conforming to the rules adopted by the players.

Further support may be drawn, lastly, from Hare's strong inclination to regard propositions expressing the pre-propositional knowledge of his dancers as synthetic *a priori*. Any conjunction

[2] *Scotland's Dances*, London, 1954, pp. 45 ff.
[3] Princeton, 1947, pp. 48 ff.

of rules, such as those which together constitute the eightsome reel, is *non-analytic* in the Kantian sense, since its negation is not self-contradictory; and again it is *a priori* in the Kantian sense, since it is logically independent of all propositions describing sense experience. But, however little one may think of the explanatory power of the notion of synthetic *a priori* propositions, Kant considered only true propositions to fall under it; and rules are not true, any more than they are false.

What has been said so far helps to clarify the relation which holds between the eightsome reel—a set of rules—on the one hand, and danced eightsome reels—performances—on the other. Let us call a performance *characteristic* if, and only if, it conforms to the rules or, what comes to the same thing, if it has all the empirical characteristics required by the rules; and let us call it *relevant* if its performers intend to satisfy the rules by it. These two features (unlike Mr. Hare's above-mentioned two conditions) are logically independent, since a relevant performance may be uncharacteristic, and a characteristic performance irrelevant—as, e.g., in case it were purely accidental.

Now a performance is correct if it is relevant and characteristic; if it is relevant but uncharacteristic, it is incorrect. There may be, as I said earlier, border-line cases: because the characteristics, being empirical, admit of them. Again, a relevant performance (an eightsome reel) may be so uncharacteristic (so badly danced) as to be properly called "chaotic," the difference between chaotic and not, amongst uncharacteristic performances (badly danced eightsome reels), being one of degree only. More important, we see that 'eightsome reel' (the set of rules) is not the genus of a species 'correctly danced eightsome reel' (relevant performance which is characteristic, i.e., conforms to the rules). Further, we cannot know how the eightsome reel is danced (the rules) without knowing how it is danced correctly: for knowing the rules implies being able to distinguish between correct and incorrect performances—this, of course, as we have seen, even though the ability to make the distinction does not in turn imply any ability to *formulate* the rules.

Summing up Part I, I should like to emphasize that trying to discover and to formulate the correct way of dancing a dance, playing a game, using words, etc., when successful, results (a) in the formulation of rules which, not being true or false, are neither empirical nor analytic propositions; (b) in the formulation of empirical propositions to the effect that somebody has adopted certain rules, that somebody's performance is or is not characteristic, or that it is or is not relevant with respect to these rules; lastly, (c) in the formulation of analytic propositions, e.g., that the eight-

some reel (= a certain set of adopted—formulated or unformulated —rules) is danced as follows, viz., . . . (= is conformed to by performances having the characteristics required by the rules. . .). I call this proposition "analytic" because it refers twice to the *same* rules: once by the phrase "the eightsome reel" and the second time explicitly; and because in stating the proposition one is stating this identity of the rules with themselves. Before, however, Mr. Hare's dancers can formulate such analytic propositions they must have succeeded in formulating propositions of the kind mentioned under (a) and (b). It is these propositions—rather than analytic propositions of which they are constituents—which express the dancers' pre-propositional knowledge.

II

3. Some crucial philosophical questions about exhibition-analysis

I propose to call the philosophical method which aims at the discovery and exhibition of rules for the use of words exhibition-analysis. In order to show its limitations I shall, first of all, ask some philosophical questions about its own scope, which, it seems to me, cannot be answered by means of the method.

How far does a system of linguistic rules depend on extra-linguistic facts? It might, for example, be argued that the satisfiability of *any* set of linguistic rules—English, ordinary Chinese, the second-order functional calculus—depends on similarities given in perception. It is such similarities which enable us to adopt rules for the assignment and refusal of signs to unlimited or open classes of objects; and unless such referential rules are adopted other linguistic rules are not satisfiable. Whatever the merits or demerits of arguments of this kind, which in any case demand more space than a paragraph, it is clear that exhibition-analysis can neither support them, nor yet weaken them, in any decisive way.

How far do systems of linguistic rules admit of alternatives? Two incompatible rules ('Only one hand to be lifted,' 'Both hands to be lifted')—unlike two incompatible indicative propositions—may both be satisfiable and each of them may be separately adopted by different groups of speakers. No criterion can be given by exhibition-analysis, which could decide whether or not the rules which it discovers are "provincial." (It is as if we could not decide whether we are the only dancers of the eightsome reel or whether there are others—whether perhaps all human beings are, apart perhaps from very small children and imbeciles.)

Connected with the above is the further question: How far, within the limits of extra-linguistic fact and the possible adoption

of alternative sets of linguistic rules, is the actual adoption of one rather than another of these systems a matter of choice? Here again a statement to the effect that "the community to which *we* belong" has adopted the following rules . . ., misses the question.

Lastly, why, if at all, should the rules discovered by some practitioner of exhibition-analysis to have been the ones adopted by himself and certain other members of his community be preferred to the rules which some other philosopher has adopted and is proposing for adoption? Assume, as we may well do, that Kant, besides having lived in the Western speech-community, has studied the science of his day, and that our analyst has not done so. Suppose that the latter is using the term 'cause' in accordance with rules different from Kant's, and assume finally that both have succeeded in formulating these rules. Unless we make some further assumption, we have not the slightest reason to prefer our analyst's rules to those of Kant. The assumption that the former is in possession of an Ideal Language has long lost its *vogue*. A more recent attempt at justifying a preference for the results of exhibition-analysis is due to Professor J. L. Austin. He argues that "our common stock of words embodies all the distinctions men have found worth drawing, and the connexions they have found worth marking, in the life time of many generations" and that these are "likely to be more numerous, more sound, since they have stood up to the long test of the survival of the fittest, and more subtle, at least in all ordinary and reasonably practical matters, than any that you or I are likely to think up in our armchairs of an afternoon—the most favoured alternative method."[4]

Even if I were to grant that only our analyst draws on the "common stock of words" and not also Kant, I find Austin's remarks, here, quite unconvincing. But merely to quote them fully is sufficient for my present purpose; it shows at least how fully aware one leading practitioner of exhibition-analysis is that the method stands in need of a philosophical justification which lies outside its scope.

4. Exhibition and replacement of rules

Dances, games, linguistic behavior, and similar activities are not only governed by rules; they also serve various purposes or ideals —the promotion of health, of enjoyment, man's technical domination of his environment, etc. Different performances, though equally correct, may serve their purpose (if they have a purpose) more or less efficiently. Indeed in many spheres of rule-governed activity it happens that the rules themselves are deemed to need

[4] *Arist. Soc. Proc.*, 1956/57, p. 8.

modifications in some respects, while being preserved in others, if a prevalent purpose of the activity is to be served by it efficiently or more efficiently.

Thus the analytic philosopher—perhaps after a good deal of exhibition-analysis—finds himself faced with rules for the use of words such as, e.g., "cause," "substance," "probable," "infinite," "justice"—rules which from the point of view of his tacitly adopted or loudly proclaimed philosophical ideals deserve preservation in some features and modification in others. The features which are in general to be modified may find expression in corresponding criteria of defectiveness; the features which are in general to be preserved may find expression in the explicit demand for a relation between the original rules and the modified ones—an analyzing relation or relation of replacement. Examples of criteria of defectiveness are vagueness, internal inconsistency, metaphysical commitments of an undesirable kind. Examples of required replacement-relations are bilateral formal implications, bilateral intensional implications of various strictness, a more or less clearly demarcated similarity.

A brief and schematic description of problems belonging to what might be called replacement-analysis could be put in this way: given certain criteria of defectiveness and a replacement relation—to replace a defective set of rules by another which is not defective and stands in the replacement-relation to the original set of rules.

The works of ancient and modern philosophers abound with analyses of this kind. Among its more recent practitioners, we find not only philosophical logicians and philosophers of mathematics but also, e.g., moral philosophers.[5] Quite often, and very naturally, putative exhibition-analyses are really examples of replacement-analysis, with both the criteria of defectiveness and the replacement-relation hidden from everybody, including the analyst himself.

This is not the place for contrasting in detail the problems and answers of exhibition-analysis with those of replacement-analysis.[6] That these are very different is, however, sufficiently evident without this. In particular, the answer to a problem in replacement-analysis is a hypothetical statement, the antecedent of which indicates the assumed criteria of defectiveness and the assumed replacement-relation. The answer to a problem in exhibition-analysis is the categorical statement that the rules formulated by the analytical philosopher have been accepted by him and others.

My attempt to show the narrowness of a philosophy limited to

[5] See, e.g., Sidgwick's *Methods of Ethics*, London, 1907, Chapter V.

[6] What I should like to say about this will be found in an essay in the volume on Broad in the Library of Living Philosophers.

exhibition-analysis is not meant to imply that this method may not lead to illuminating results. Its practice could, indeed, be quite as trivial as the activity which Bertrand Russell recently described in a philosophical fable with a moral.[7] But I would not say that it need be.

S. KÖRNER

BRISTOL UNIVERSITY

AMERICAN PHILOSOPHICAL ASSOCIATION

EASTERN DIVISION

FIFTY-FOURTH ANNUAL MEETING

HARVARD UNIVERSITY, DECEMBER 27–29, 1957

PROGRAM

FRIDAY, DECEMBER 27

2:00 P.M.

I. JOINT SESSION WITH THE ASSOCIATION FOR SYMBOLIC LOGIC. *Burr Hall, Room B.*

Paul Lorenzen: "Formalism and Logical Reflection."

Richard M. Martin: "Toward a Systematic Pragmatics."

Other papers to be announced.

Chairman: S. C. Kleene

II. HISTORY OF PHILOSOPHY (MODERN). *Burr Hall, Room A.*

Willis Doney: "Descartes' Theory of Vision." Commentator: Jerome Shaffer.

Norman Kretzmann: "Maupertuis, Wittgenstein, and the Origin of Language." Commentator: John O. Nelson.

Colin M. Turbayne: "Berkeley's Two Concepts of Mind." Commentator: Brand Blanshard.

Chairman: Paul Weiss

III. PHILOSOPHY OF MAN. *Emerson Hall, Room 211.*

Rudolf Allers: "Reflections on the Ontology of the Artifact." Commentator: C. J. Ducasse.

Hans Jonas: "On the Differentia of Man." Commentator: Joseph Katz.

[7] This JOURNAL, Vol. LIV (1957), No. 9.

Lawrence W. Beals: "Can an Institution be 'Unnatural' if 'Man Has No Nature'?" Commentator: Rubin Gotesky.

Chairman: John Ladd

IV. Ethics. *Emerson Hall, Room D.*

Philip Nochlin: "Ethical Scepticism." Commentator: George Nakhnikian.

Ethel Albert: "'First Principles' in Logic and Ethics." Commentator: Stuart M. Brown, Jr.

Mary Mothersill: "Hedonism and the 'Lower' Pleasures." Commentator: Bernard Wand.

Chairman: Abraham Edel

8:00 P.M.

Smoker. *Memorial Hall* (Cambridge and Kirkland Streets).

Saturday, December 28

9:00 A.M.

I. Justice as Fairness (Symposium). *Burr Hall, Room B.*

John Rawls and Everett W. Hall. Commentators: Arthur E. Murphy and William Frankena.

Chairman: Maurice Mandelbaum

II. The Evidence for Aesthetic Judgment (Symposium). *Emerson Hall, Room D.*

Mortimer E. Kadish and Albert Hofstadter. Commentators: Frank Sibley and Monroe C. Beardsley.

Chairman: Vincent Tomas

III. History of Philosophy (Ancient and Medieval). *Burr Hall, Room A.*

John W. Lenz: "Looking Back at 'The Sea Fight Tomorrow.'" Commentator: J. J. C. Smart.

Paul Oskar Kristeller: "Averroism and Alexandrinism in the Light of Recent Studies." Commentator: Arthur Hyman.

Chairman: Peter A. Bertocci

IV. Philosophy of Science. *Emerson Hall, Room 211.*

T. A. Goudge: "Causal Explanations in Natural History." Commentator: Nicholas Rescher.

E. H. Madden: "The Riddle of Induction." Commentator: Wesley C. Salmon.

Chairman: Roderick Chisholm

11:30 A.M.

Business Meeting. *Emerson Hall, Room D.*

2:00 P.M.

I. SUBSTANCE AND FORM IN ARISTOTLE (Symposium). *Burr Hall, Room B.*

Wilfrid Sellars and Rogers Albritton. Commentators: J. H. Randall, Jr., and Joseph T. Clark.

Chairman: Gregory Vlastos

II. LOGIC AND LANGUAGE. *Burr Hall, Room A.*

Arthur Pap: "Types and Meaninglessness." Commentator: Max Black.

E. M. Adams: "On Knowing That." Commentator: Hilary Putnam.

Chairman: Rulon Wells

III. ETHICAL ANALYSIS. *Emerson Hall, Room A.*

John W. Blyth: "Analyzing Ethical Terms." Commentator: Elizabeth Flower.

Hector N. Castañeda: "Hypothetical Imperatives." Commentator: Harry Tarter.

Chairman: Roderick Firth

IV. AESTHICS. *Emerson Hall, Room 211.*

Milton C. Nahm: "Originality and the Aesthetics of Criticism." Commentator: Morris Weitz.

Joseph Margolis: "The Problem of Relevance in Aesthetic Criticism." Commentator: David Sachs.

Chairman: John Hospers

4:45 P.M.

TEA. *Harvard Faculty Club* (20 Quincy Street).

7:00 P.M.

BANQUET. *Eliot Dining Room* (at the foot of Dunster Street).

PRESIDENTIAL ADDRESS BY WILLARD V. QUINE: "Speaking of Objects."

Maurice Mandelbaum, *Presiding*

SUNDAY, DECEMBER 29

9:30 A.M.

I. DETERMINISM IN THE LIGHT OF RECENT PHYSICS (Symposium). *Burr Hall, Room B.*

Norbert Wiener, Adolf Grünbaum, and Michael Scriven. Commentator: Carl G. Hempel.

Chairman: Willard V. Quine

II. The Nature of Analysis (Symposium). *Emerson Hall, Room D.*

R. M. Hare, Paul Henle, and S. Körner. Commentator: Charles A. Baylis.

Chairman: Victor Lowe

11:30 A.M.

Conference on Philosophy in Education. *Emerson Hall, Room D.*

Report on "New Developments in Education and Problems Concerning the Profession and Teaching of Philosophy," by Charles W. Hendel, Chairman of the Standing Committee on Philosophy in Education of the American Philosophical Association. The Report will be followed by general discussion.

GROUP MEETINGS

Friday, December 27

9:30 A.M.

Creative Ethics Group. *Emerson Hall, Room 211.*

Symposium: *Metaphysical Bases of Ethics.*

Harold H. Titus: "Can We Get Together on Basic Moral Postulates?"

Daniel D. Williams: "Moral Obligation in Process Philosophy."

Hugo A. Bedau: "Advising, Justifying, and Understanding."

Comments by James Gutmann.

Chairman: William S. Minor

10:00 A.M.

The Peirce Society. *Emerson Hall, Room A.*

Vincent Tomas: "Peirce's Dispositional Concept of Belief."

John E. Smith: "Interpretation, Intuition, and Self-Knowledge."

W. E. Schlaretzki: "Scientific Inquiry and the *Summum Bonum.*"

Chairman: Philip P. Wiener

7:00 P.M.

I. Association for Realistic Philosophy. *Emerson Hall, Room H.*

T. Foster Lindley: "Some First Principles and Causes Today."

Chairman: Robert C. Baldwin

II. Personalistic Discussion Group. *Emerson Hall, Room F.*

Warren E. Steinkraus: "Berkeley's Wisdom on Other Minds."

Chairman: Peter A. Bertocci

III. Society for Ancient Greek Philosophy. *Emerson Hall, Room A.*

Gregory Vlastos: "Zeno's First Argument Against Plurality."

Donald C. Williams: "Form and Matter."

Chairman: Raphael Demos

THE AMERICAN SOCIETY FOR POLITICAL AND LEGAL PHILOSOPHY

ANNUAL MEETING

Cambridge, Massachusetts, December 29–30, 1957

PROGRAM

I. Jacob Taubes: "Community in Historical Perspective."
Discussion by George Catlin.

II. Herbert Schneider: "Community, Communication, and Communion."
Discussion by Benjamin Lippincott.

III. Wolfgang H. Kraus: "Requirements of the Democratic Community and the Problem of Government Secrecy."
Discussion by Lon L. Fuller.

IV. Huntington Cairns: "The Idea of Community in Jurisprudence."
Discussion by Stuart M. Brown, Jr.

V. Talcott Parsons: "Community in Recent Sociological Theory."
Discussion by Frank H. Knight.

NEW BOOKS

ANSCOMBE, G. E. M.: *Intention.* Oxford: B. Blackwell, 1957. ix, 93 p. 10s. 6d. *Also* Ithaca, N. Y.: Cornell Univ. Press. $1.50.

BRENTANO, FRANZ: *Die Lehre vom richtigen Urteil.* Nach den Vorlesungen über Logik mit Benützung anderer Manuskripte aus dem Gebiete der Erkenntnistheorie aus dem Nachlass hrsg. von Franziska Mayer-Hillebrand. Bern: Francke, 1956. xxiv, 344 p. Bound, 26.50 Sw. fr.; paper, 22.–Sw. fr.

BULLOUGH, EDWARD: *Aesthetics; Lectures and Essays.* Ed. with an Introd. by Elizabeth M. Wilkinson. Stanford, Calif.: Stanford Univ. Press [1957]. xliii, 158 p. $4.50.

CASSIRER, ERNST: *The Philosophy of Symbolic Forms.* Vol. III: *The Phenomenology of Knowledge.* Tr. by Ralph Manheim. Introductory Note by Charles W. Hendel. New Haven: Yale Univ. Press; London: Oxford Univ. Press, 1957. xvii, 501 p. $8.50.

GREENE, THEODORE MEYER: *Liberalism, Its Theory and Practice.* Austin: Univ. of Texas Press [1957]. x, 219 p. (The Rushton Lectures.)

JOHNSON, F. ERNEST, ed.: *Patterns of Faith in America Today.* New York: Institute for Religious and Social Studies; Distributed by Harper & Brothers [1957]. 192 p. (Religion and Civilization Series.) $3.00.

JONES, HOWARD MUMFORD: *American Humanism; Its Meaning for World Survival.* New York: Harper [1957]. xvi, 108 p. (World Perspectives, Vol. 14. Planned and ed. by Ruth Nanda Anshen.) $3.00.

KAUFMANN, FRITZ: *Thomas Mann: The World as Will and Representation.* Boston: Beacon Press [1957]. xiv, 322 p. $6.00.

KOOPMANS, TJALLING C.: *Three Essays on the State of Economic Science.* New York [etc.]: McGraw-Hill, 1957. xi, 231 p. $6.50.

LUCE, R. DUNCAN, AND RAIFFA, HOWARD: *Games and Decisions; Introduction and Critical Survey.* A Study of the Behavioral Models Project, Bureau of Applied Social Research, Columbia University. New York: John Wiley; London: Chapman & Hall [1957]. xix, 509 p. $8.75.

MCKEON, RICHARD; MERTON, ROBERT K.; GELLHORN, WALTER: *The Freedom to Read; Perspective and Program.* New York: Pub. for the National Book Committee by R. R. Bowker Co., 1957. xvii, 110 p. $2.50.

MASCALL, E. L.: *Words and Images; a Study in Theological Discourse.* New York: Ronald Press [1957]. xi, 132 p. $3.50.

ORTEGA Y GASSET, JOSÉ: *Man and People.* Authorized Translation from the Spanish by Willard R. Trask. New York: W. W. Norton [1957]. 272 p. $4.50.

The Oxford Dictionary of the Christian Church. Ed. by F. L. Cross. London [etc.]: Oxford Univ. Press, 1957. xix, 1492 p. $17.50.

PEARS, D. F., ed.: *The Nature of Metaphysics.* London: Macmillan; New York: St. Martin's Press, 1957. vi, 164 p. $2.50.

PLATO: *Statesman.* Tr. by J. B. Skemp. Ed., with an Introd., by Martin Ostwald. New York: Liberal Arts Press [1957]. xxxvi, 100 p. (The Library of Liberal Arts, No. 57.) Paper, $0.75.

SHKLAR, JUDITH N.: *After Utopia: The Decline of Political Faith.* Princeton, N. J.: Princeton Univ. Press, 1957. x, 309 p. $5.00.

SINGER, IRVING: *Santayana's Aesthetics, a Critical Introduction.* Cambridge, Mass.: Harvard Univ. Press, 1957. ix, 235 p. $4.75.

SOROKIN, PITIRIM: *Social & Cultural Dynamics; a Study of Change in Major Systems of Art, Truth, Ethics, Law and Social Relationships.* Rev. and abridged in one volume by the author. Boston: Porter Sargent [1957]. 718 p. (Extending Horizons Books.) $7.50.

STRASSER, STEPHAN: *The Soul in Metaphysical and Empirical Psychology.* Pittsburgh, Pa.: Duquesne Univ.; Louvain, Belgium: E. Nauwelaerts, 1957. x, 275 p. (Duquesne Studies, Philosophical Series, 7.) Bound, $5.00; paper, $4.25.

TATON, R.: *Reason and Chance in Scientific Discovery.* Tr. by A. J. Pomerans. New York: Philosophical Library [1957]. 171 p. $10.00.

WOLFF, HANS M.: *Plato: Der Kampf ums Sein.* Berkeley & Los Angeles: Univ. of California Press, 1957. 312 p. (Univ. of California Publications in Philosophy. Vol. XXX.) $4.50.

WRIGHT, GEORG HENRIK VON: *Logical Studies.* New York: Humanities Press; London: Routledge [1957]. ix, 195 p. (International Library of Psychology, Philosophy and Scientific Method.) $5.00.

NOTES AND NEWS

We note with great pleasure that Professor William Ernest Hocking has won the 1957 Lecomte du Nouy Award for his book, *The Coming World Civilization.*

THE INTERNATIONAL INSTITUTE OF PHILOSOPHY

announces the publication of Vol. IV (1957) of its *Quarterly Bulletin*

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